HORTICULTURAL SOCIETY OF NEW YORK

(INCORPORATED 1902)

MEMOIRS, VOL. I.

PROCEEDINGS

International Conference

ON

Plant Breeding and Hybridization

1902

Held in the Rooms of the American Institute of the City of New York
And in the Museum Building of the New York Botanical Garden

SEPTEMBER 30 and OCTOBER 1 and 2

Horticultural Society of New York

(INCORPORATED)

1902

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COMPLIMENTARY EXCURSION

On the day following the last business session of the Conference the Council of the Horticultural Society of New York invited the delegates and members of the Conference to an excursion up the Hudson River, under the guidance of Mr. James Wood, president, who explained the various points of interest passed en route. A landing was made at Poughkeepsie, where conveyances met the party, and Fern Tor, the home of Mr. F. R. Newbold, treasurer of the Society, was visited, by invitation. Here the party was entertained, and, after a short rest, the estate of Mr. F. W. Vanderbilt, at Hyde Park, was visited, Mrs. Vanderbilt receiving the visitors, and accompanying them around the gardens and farm. This place is notable as the site of Dr. Hosack's old home, where many fine specimen trees are to be seen. The party returned to New York City by train.

The hearty thanks of the Conference Committee is due to Mr. Newbold and the Misses Newbold for their very hospitable reception and entertainment.

The following persons attended this excursion:

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PAPERS PRESENTED AT THE CONFERENCE

Thursday, October 2

APPENDIX—PAPERS READ BY TITLE

INTERNATIONAL CONFERENCE ON PLANT BREEDING AND HYBRIDIZATION

The first session of the conference was called to order at ten o'clock A. M. on Tuesday, September 30, 1902, by President James Wood of the Horticultural Society of New York, who without formality opened the proceedings and stated that the Horticultural Society had made arrangements for the publication of the proceedings of the conference, the contents of the volume to be coprighted as a whole by the Society, but reserving to the authors exclusively all rights in the papers which they present.

The following paper was then read by W. Bateson, of Cambridge University, England:

PRACTICAL ASPECTS OF THE NEW DISCOVERIES IN HEREDITY

By W. Bateson, Cambridge University, England.

Mr. President, Ladies and Gentlemen: It is impossible for me to begin the serious discussion of these subjects without expressing the pleasure that I feel in having this opportunity of addressing you. It is and must be always a great pleasure to a man who is engaged in a very special line of inquiry, as breeding experiments are, to meet others who are engaged in such work, whose thoughts are centred on the same problems as his own. Especially may I welcome this opportunity of speaking here in the United States, where what is being done in this line of inquiry is on a scale of comprehensiveness which I may truly say far exceeds anything that is being done in any other country in the world. We have only to glance at the publications of the agricultural experiment stations to know what progress is being made in this line. Here amid vast diversities of soil and climate the great resources of the States are being applied to the elucidation of these problems, with the result that the scope of the work carried on entirely surpasses that which is attempted by other nations. It is therefore with especial satisfaction that I welcome the opportunity of addressing those who in the United States are devoting themselves to the study of experimental breeding.

In these studies we have reached a critical moment. That crisis, as it is known to many of those present, has been brought about by the rediscovery and confirmation of Mendel's work on heredity. These discoveries intimately concern the art of the practical breeder, and I propose to use the present opportunity to indicate some of the ways in which we can employ them for his purposes.

The essential point which Mendel discovered in peas, and others are now discovering in various fields of inquiry, is that, though a plant or an animal may be made up of a great complex of characters, height, size, color, hairiness, form of fruits and organs, etc., yet in a very considerable number of cases, a number which increases almost every month, those characters may possess an individuality manifested in the formation of the germ cells. When two varieties differing in, say, color, or form, or hairiness, or whatever it may be, are crossed together a "hybrid" is formed. That hybrid when it comes to make its own germ cells, male cells, or female cells, makes them in a number of cases, indeed in all Mendelian cases, such that each germ cell represents *one* of the pure grandparental characters, and not both. That is the essential discovery of Mendel.. The cases that are most familiar are those in peas, the subject on which he originally worked. If a pea with green cotyledons be crossed with one having yellow cotyledons a hybrid is produced. That hybrid grows up and bears peas in its turn. Those peas will be composed, each individual pea, of a union of two germs, each germ being a carrier of either one or the other of the pure parental characters. Therefore we may have two green germs uniting, or two yellow germs uniting, or a yellow germ uniting with a green. Each gamete in such a case is pure to one or the other of the two parental characters which you first put into the hybrid. In other words, we can recognize many different characters in animals or plants which are unit characters, and in the formation of gametes are treated as distinct entities or units.

If, instead of using pure parental forms differing from each other in respect of *one* pair of antagonistic—allelomorphic characters, as we call them—we use parental forms distinguished in respect of two, three or more pairs of allelomorphs, then each germ cell in Mendelian cases will contain or transmit *one character only of each pair.*

To use an illustration: In chemistry you may have a body, say, a simple salt, from which you can take out the base, or the acid radical, replacing the base by another base, or the acid radical by another acid radical. You can in that way decompose your substance into component parts, reforming them in various combinations. So we must imagine a plant which has one element of color, for example, another element of texture, etc., and we must conceive that when two varieties are crossed together the unit characters can be combined and recombined in the gametes of the hybrid, alternating with and replacing each other by substitution. You can take out greenness and put in yellowness; you can take out hairiness and put in smoothness; you can take out tallness and put in dwarfness, etc. The characters have their fixed possibilities of union, and hence it may be possible for us to form some mental picture of the constitution of the organism.

Now when we come to the question of the significance of these things to the breeder or to the hybridist, it will be found that the significance is exceedingly great. I am afraid of saying that we have already reached a point when the practical man who is doing these things with a definite, economic object or commercial object in view can take the facts and use them for his definite advantage. But we do for the first time get a clear sight of some of the fundamentals on which he will in future work, and it cannot be now very

many years, if the investigations go on at the present rate, before the breeder will be in a position not so very different from that in which the chemist is:—when he will be able to do what he wants to do, instead of merely what happens to turn up. Hitherto I think it is not too much to say that the results of hybridization had given a hopeless entanglement of contradictory results. We did not know what to expect. We crossed two things; we saw the incomprehensible diversity that comes in the second generation; we did not know how to reason about it, how to appreciate it, or what it meant. We got contradictory results, and the thing looked hopeless. But with the discovery of the purity of the germ cells we have the first step, which, I think, is bound in a very short time to become a path through many of those wonderful mazes of heredity.

To the practical man, I take it, the importance of this discovery comes in, first, somewhat as follows: Seeing that the gametes are pure with respect to their characters, it follows that an individual which is produced as the offspring of a cross will be composed, in respect to any one pair of these characters, either of two similar gametes or of two dissimilar gametes. Take the case of the pea. Any one pea descended from an original cross between yellow and green will *either* be composed of two similar green gametes, *or* of two similar yellow gametes, *or* of a yellow and a green. Now, as it happens in the case of the pea, and in a great number of other cases, unfortunately for the breeder, there is no means of distinguishing outwardly by any test that we can apply whether the organism is a hybrid or pure to the *dominant* character. There is no way of distinguishing in the cases where yellow meets green whether the organism is a hybrid that is composed of yellow and green, or whether it is pure to the yellow character and is composed of two yellow gametes. There is no possibility of distinguishing, because the yellow is, as Mendel calls it, dominant, and the green is hidden, or, as he calls it, recessive. We have, therefore, in such a case as that, two classes of organisms, pure and hybrid, each showing the dominant character, and it is owing to the fact that the pure dominant cannot be distinguished from the dominant hybrid containing both dominant and recessive characters that an immense number of the contradictions which the practical breeder experiences have come about. For example, a breeder or seedsman introduces some strain of a new variety of his seed—peas, or whatever it may be. He finds a number of rogues which are not true to the character which he desires to put on the market—rogues which he is unable to eliminate. Formerly we said it was only a question of time; he must hoe out the rogues and go on, and he will gradually fix his type. But now we begin to see what the facts really mean. He hoes out the rogues, and again they come—in diminishing number, no doubt, but they are still there. We now suspect the nature of such rogues in a considerable number of examples. For example, the bearded wheats occurring among wheats which are intended to be beardless. Every year they grow, and every year the seedsman hoes them out, and again they come back. Those bearded wheats may come from the fact that the beardless wheats had a bearded ancestor, and some of them contain bearded germs. If you cross a bearded wheat with a beardless wheat, the first cross will be a beardless wheat. You allow it to fertilize itself, and you sow your crop; you begin to get beards and

beardless. You take out the bearded, and again there will be a beardless crop with a certain number of bearded. To get your crop pure in a few generations you should make your selection from *individual* plants. Then you will begin to find that some plants carry only the bearded character and some will carry both. It is the coming out of these recessive characters, owing to the fortuitous union of recessive germs, which shows itself in the offspring you desire to get rid of.

Whenever, then, it is desired, in a crossbred strain, to fix a *dominant* character selections must always be made of *single families* containing no recessive members.

We reach, therefore, a fact of immediate interest to the practical breeder. We have lost forever, I think, the conception that fixity of character is solely or chiefly a function of the number of generations during which that character has been manifested, or of the number of successive selections of that particular variety which have been made. *Purity of strain or fixity of character is, on the contrary, due primarily to the union of similar gametes in fertilization.* Such purity may therefore occur among the immediate offspring of crossbred organisms.

Another question of considerable practical significance is that of the nature and causation of dominance, involving the further question whether the breeder has any means at his disposal by which dominance may be created, modified or controlled. Upon this point experimental results are still to seek, and though there are a few cases* where we know that the dominance of one character over another varies in intensity, we have no clear indication as to the causes governing these differences of intensity. We may naturally be disposed to consider whether continued pure-breeding, or, perhaps, in-breeding, may not be concerned in the creation of dominance, but the facts at present ascertained give no clear light on this question. We have, however, abundant evidence that pure breeding is not essential to the constitution of dominance; for in any simple Mendelian case the pure dominants, offspring of one crossbred and one dominant parent, or of two crossbred parents, may, and commonly do, show unimpaired dominance over recessives of pure lineage.

But there is another class of facts which, to my thinking, is far more interesting than that, and is of more significance to the practical breeder, and that is this: I spoke in the case of the green and the yellow pea of the offspring resembling the dominant, the yellow. But in a great number of cases we find a phenomenon not nearly so simple as that. When similar germs meet they produce a pure bred organism, which in my terminology is called a *homozygote,* a yoking together of like germs. When the germs are dissimilar they make a new form, a hybrid form, which in this terminology we may call a *heterozygote,* the yoking together of two dissimilar germs in the zygote form. Until we have seen the heterozygote, its form is not predicable in any specific case. You cannot say until you have made the specific cross what the character of that heterozygote will be. It may be that, through dominance, one character only prevails to the exclusion of the other, or the heterozygote may have some form totally distinct from that of either of the parents.

*The extra toe of the Dorking fowl, for instance, is uniformly dominant in some strains, but not in others.

For example, in a case that I see a great deal of, in the sweet pea, you may by crossing two sweet peas produce the old purple sweet pea with chocolate colored standards and purple wings. That purple sweet pea so produced will not breed true. The old purple pea of the gardeners was a pure pea and would perpetuate itself truly from seed; but the purple pea produced as a heterozygote form will not breed pure, but will split up into the components which produced it. So that we recognize that there is a new form, a heterozygote form, which, though it may resemble some pure form, will not breed true. This is a case that may not interest the seedsman, because he does not want the old sweet pea. Nevertheless, in his fertilizations he may produce another new form which he does want, and after all his laborious selection he may find it is only a heterozygote which will never breed true.

It is a curious and unexplained fact—constituting one of the most fertile fields of inquiry—that when dissimilar gametes meet they should so often produce an ancient form. That is what we now recognize as the *rationale* of Darwin's "reversions on crossing." When Darwin crossed his pigeons he brought back an old form; and so in crossing many plants you can get back a reversionary form by uniting two dissimilar gametes.

In my own experience a most extraordinary case of this nature has occurred. When the Mendelian discoveries were first announced it was obviously desirable to cross two varieties differing in some visible character of the gametes (whether alike in other respects or not). By such means we might hope to make visible that mixture of dissimilar gametes which must certainly occur in Mendelian hybrids. Unfortunately the actual *gametes* of flowering plants are not adapted to this experiment, but the nearest things to them are the pollen grains. So I cast about for a case of visible variation in pollen. By good fortune I found them at once in a certain sweet pea.

Ordinary sweet peas have their pollen grains elongated, with three pores. The white variety known as *Emily Henderson,* an American sort about eight to ten years old, usually has pollen grains which, when treated with acids, etc., are seen to be roughly spherical, with only two pores. Various grains of intermediate types are found from time to time in the pollen of *E. Henderson,* and the round grains not very rarely have three pores. But the pollen of a round-pollened plant can generally be distinguished immediately from that of a long-pollened plant.

Proposing, then, to cross *E. Henderson* with sweet peas having typical pollen, I sowed a quantity of that variety. But when the plants flowered I discovered that, though a majority of the *Hendersons* had round pollen, a few, though otherwise indistinguishable from the others, had nevertheless long pollen, exactly like a common sweet pea. I then crossed the round pollened *Henderson* with the long, and *vice versa.* The same experiment was also made independently by Miss E. R. Saunders. *Every seed then produced (from four capsules) has given a plant with chocolate-purple standards and blue-purple wings!* There are many details respecting this remarkable case which I hope ere long to publish, but I mention it now as illustrating in a striking way how paradoxical are the phenomena empirically produced by the experimental breeder, and how puzzling are these heterozygous forms.

I may say that my experiment entirely failed to fulfil its original purpose,

which was to see a mixture of true long grains and true round grains, for all the pollen of these plants was typically long, showing dominance of the long pollen as a plant character.

I know no example of the production of an atavistic heterozygote so curious as this. It should be stated that white color of flowers is, in general, a pure recessive character in sweet peas. Hence, had I happened to cross a long and a round pollened *Henderson* together without knowing of this pollen difference, I should not have been aware of the heterozygous nature of the purple mongrel, and should have been still more hopelessly unable to bring the facts into line. Even as it stands, we may feel fairly sure that something more than simple Mendelian phenomena are presented by this case, but pending the next generation we cannot analyze it any further.

The occurrence of these heterozygous forms concerns the practical breeder very closely. The breeder may breed a new variety of value, and he may be most anxious to obtain its seed pure. Year by year he selects it, but every year, if it be a heterozygote, it fails to come true; because, as we now see, its germ cells do not transmit or represent the heterozygous character, but merely the pure characters of its components.

In the garden of my friend, Mr. Sutton, of Reading, I have seen a case of this kind. It is a beautiful Chinese Primrose (*Primula sinensis*) of a curious lavender color. The seeds of the self-fertilized lavenders are sown each year, but of the total offspring only about half are lavenders, one-quarter being a tinged white and one-quarter magentas. We can scarcely doubt that the lavenders are formed as the heterozygote of that particular white and magenta, the whites being homozygotes formed by the union of two white gametes, while the magentas are similarly formed by the union of two magenta gametes. In such a case statistical study of the offspring will show the breeder with approximate certainty what he is dealing with, and will give him a good indication whether it is worth while for him to continue in his attempt to get the variety true.

A case almost certainly of the same nature occurs in poultry—the case of the Andalusian fowl. The Andalusian was at one time a favorite breed. Its plumage is of a peculiar blue-gray, mixed with black. You may go to the poultry shows and buy the winning Andalusians, thinking that they will breed true. But they will not. Andalusians have been bred for at least forty or fifty years, and there is no good reason for thinking that they breed any truer now than formerly. Every one is agreed that the breed possesses this drawback. The "impurity" manifests itself in the production of numerous black birds and numerous white birds irregularly splashed with blackish gray. From such evidence as I can obtain it seems almost certain that these two objectionable forms are produced in about equal numbers, and that the number of true Andalusians is about double the number of either. The Andalusian is almost unquestionably a heterozygous form made by the union of the black gamete with the white-splashed gamete. It is, moreover, on record that the two sport-forms crossed together produce *only* Andalusians, as they should do if the case is a simple Mendelian one. We may, therefore, predict that the Andalusian, like the lavender Primula, will *never* breed true, however well or long it be selected.

In these brief remarks I have indicated some of the lines along which the Mendelian discoveries will have a close bearing on the work of the practical breeder. We have for the first time a conception of the true nature of at least a part of the facts which underlie the outward and visible phenomena witnessed by the breeder. As I have attempted to show, we have at last a clear notion of the meaning of purity or fixity of type, of the consequences of dominance and of the nature of heterozygous forms—phenomena which go to make up the daily experience of those who are practically engaged in these pursuits. It is impossible on the present occasion to go into many other fascinating problems suggested by these simple facts. For example, we do not proceed far with the practice of experimental breeding before we meet the phenomenon of the decomposition or resolution of compound characters into simpler constituent characters (hypallelomorphs), themselves possessing a measure of individuality. Then again we are presented with a whole series of possibilities of the utmost consequence both to the naturalist and the practical breeder.

It is difficult to see this phenomenon of the decomposition or resolution of compound characters without feeling the conviction that we have here the key to a great part of the mystery of *parallel* variations. We are led to suspect that the series of colors, for instance, into which the original color of the Carnation has been split up may be a similar series to that into which, say, the sweetpea has been split up. We can in this way imagine that each series of component colors consists of a number of definite terms related to each other in a definite way such that, if we could ascertain the relation of yellow in the one series, we could predict somewhat similar relations for yellow in the other series. The colors of flowers give us many such series, and even classes of series, of which some have obviously distinct laws of their own. Nevertheless, it is in a high degree likely that if one such series of colors were studied statistically in such a way that what I have called the mutual relations of its terms could be stated, we should have a model which would enable us to reconstruct other similar series, to predict its terms, and possibly to set about producing them at will.

In this paper I have spoken only of the simpler deductions from Mendel's principles. To this audience I need scarcely say that we are well aware that those principles in their simple form cover only a part of the phenomena of heredity. In trying to extend them or to cast them into a general form many reservations must be made that cannot now be detailed, and a vast field must be covered by *specific* experiment before such generalizations can be successful. Chief, perhaps, of the difficulties we can at present foresee is that caused by the existence of numbers of specific heterozygotes, which may appear quite unexpectedly owing to the presence of unknown differentiations between parent-strains presumed to be identical. Such a case is that of the *E. Henderson* mentioned above. Phenomena of this kind will doubtless be found elsewhere, and will lead to great difficulties of interpretation. Against such cases the observer must be on his guard. The significance of such forms can only be studied by an analysis of their offspring.

In addition to the general development of the inquiry we may note three chief subjects that call for immediate investigation:

1. The resolution of compound characters and a statistical study of their components.
2. The nature of dominance and its possible limitations.
3. The detection of differentiation among the gametes of cross-bred organisms.

As to the last two we are still in ignorance how to proceed, but the first is a question we can at once attack by Mendelian methods.

But apart from the profounder mysteries, the unravelling of the problems of heredity has now become a matter for simple statistical research. Owing to the scale on which they must be pursued, it is likely that for their further elucidation we must perhaps look rather to the practical breeder whose operations are of large extent than to the scientific investigator whose resources are generally of a more limited character. But if in the future some cooperation between these two groups of workers can be secured, we may confidently look forward to the time when the laws of heredity, hitherto a hopeless mystery, will, in their outward presentments, at least, be, as the laws of chemistry now are, a matter of every day knowledge. The period of confusion is passing away, and we have at length a basis from which to attack that mystery such as we could scarcely have hoped two years ago would be discovered in our time.

[For a fuller account, in English, of Mendelian facts and problems, the reader is referred to the *Report to the Evolution Committee of the Royal Society* No. 1, by W. Bateson and E. R. Saunders; also to *Mendel's Principles of Heredity*, by W. Bateson, containing a translation of Mendel's papers, together with a discussion; published by the Cambridge University Press (in America, the Macmillan Company). These papers give references to the chief writings on the subject, especially those of De Vries, Correns, and Tschermak, who almost simultaneously announced the rediscovery and confirmation of Mendel's work. In the latter publication, on p. 71, 4th line from bottom, *Abab* should be *Ab, ab;* and *aBab* should be *aB, ab.* The following corrections should be made in the *Report* referred to above: p. 24. The offspring of *S. inermis* x *S. armata* should stand in the column headed "S. ar." p. 105, 2nd line. For "agree precisely, being 3.0:1" read "are 2.7:1." p. 160. *Note.* For "talls" read "*Cupids.*"]

The Chair: We feel very greatly indebted to Mr. Bateson for his admirable presentation of these principles underlying fertilization. I am sure he has brought to each one of us here who has had any practical experience in this work the explanation of some difficulties that we have run against, whatever our work may have been. And I want to commend Mr. Bateson for the admirable presentation of a subject so full of information for us.

L. H. Bailey: Mr. President, I should like to say one word in regard to this matter of the Mendelian hypothesis. I have tried to follow it myself in this last year or two. I wish to say to you that if you wish to follow this with the greatest degree of accuracy you should get Mr. Bateson's recent book, "Mendel's Principles of Heredity." I don't believe that we shall get ready for a long time to formulate laws by means of which we may predict what is coming, because our premises are as yet in a way unknown. But it seems to me that the resuscitation and revival of Mendel's theories are going to open a whole new field to speculation in regard to the principles of heredity. It seems to me that the next few years are going to see a discussion of the principles of heredity in regard to plants that is comparable to that which followed Darwin's discovery. It seems

to me it is as important as that. I expect to use this book as a basis for all our work in plant breeding.

William Saunders: Mr. President, this paper has thrown light upon many subjects which have been somewhat dark in my mind. For instance, in the cross-fertilizing of wheats we have often found that the crossing of two beardless forms will produce a bearded form, or we have a beardless wheat as the result of the crossing of two bearded forms. This explanation that Professor Bateson has given us throws light on that point and on many similar points which have puzzled many of us who are practical workers in this very interesting field.

H. H. Groff: The principles referred to by Mr. Bateson are certainly of great interest as regards primary crosses, and those crosses are related to our comparatively early experience in work of this kind. But the great question of interest to us in the future (and even now to those workers of extended experience) will be in regard to those crosses which are multiple to a limitless degree. These will present the questions in the future. It is not so much what we expect to find between hybrids or crosses containing a limited number of characteristics, but when crosses contain many thousands the problem will be far greater.

W. J. Spillman: I have with me some specimens, or rather some figures, of the specimens of wheat illustrating this law. I place them on exhibition.

The Chair: I could have presented from my own fields this season ten acres of illustration of Mr. Bateson's statement in regard to growing wheat. I have been growing a hybrid wheat for a number of years in a practical way as a farmer, and the seedsmen have taken the crops, and every year I have had to fight these bearded specimens of plants that came up in this field. To me it has been one of my greatest puzzles, as I was making no progress whatever; and while I never allowed one of those plants to go into my field, yet year after year I had the same result. I can see that it has been a bottomless work that I have been trying.

The following paper by C. C. Hurst, of England, was read by the Secretary:

NOTES ON MENDEL'S METHODS OF CROSS-BREEDING

By Charles C. Hurst, Burbage, Hinckley, Eng.

The first hybrid plant, raised by hand, appears to have been recorded by Richard Bradley, in 1717, as a cross between the Carnation *(Dianthus caryophyllus.* ♀) and the Sweet William (*D. barbatus.* ♂) ; it was raised by Thomas Fairchild, of the Hoxton Nurseries, near London. (Ref. 1.)

Since then many hybrids and crosses, in many genera, have been raised by many persons, in many countries. Among others, the names of Köbreuter, Knight, Herbert, Gartner, Godron, Nandin and Darwin stand pre-eminent. The culminating point of all these being the experiments, reasearches and broad generalizations of Charles Darwin, which mark off a distinct epoch. The new epoch seems to have begun actually in Darwin's time, though apparently quite unknown to himself and to his contemporaries.

In 1866 (about two years before Darwin published his monumental work on the "Variation of Animals and Plants under Domestication") Gregor Mendel published, at Brünn, the records of his remarkable experiments in cross-breeding distinct races of the Garden Pea *(Pisum sativum).* (Ref. 2.)

Curiously enough, this work remained in obscurity until 1900, when it was brought to light, almost simultaneously, by the experiments and researches of De Vries in Holland, Correns in Germany, Tschermak in Austria, and Bateson in England. So that, although 1866 marked the beginning of the new epoch, yet it was not until the last year of the nineteenth century that any marked advance was made. The psychological moment had apparently arrived, and during the past two years the progress in certain directions has been phenomenal. Experiments with various kinds of plants and animals, carried out on Mendelian lines, have yielded large numbers of facts, which, on the whole, practically confirm the results obtained by Mendel, though, at the same time, it is only fair to state that apparent exceptions are fairly numerous.

In face of these exceptions, and notwithstanding the many confirmations of Mendel's results by different observers in different kinds of plants and animals, it is quite possible that it is too early yet to regard Mendel's principles as capable of general application. At the same time, there is no doubt that Mendel's experiments and those of his disciples are a great advance on what has been done before, and will probably prove a stepping-stone towards the final solution of the problems of inheritance. For the present it may be wise

(1) "New Improvements of Planting and Gardening," 1717; cap. ii., p. 22.

(2) "Versuche uber Pflanzen—hybriden," abhandl. d. Naturf. Vereins in Brunn, 1866, iv., pp. 1—47. (See also English translation of above by the Royal Horticultural Society of London, in Journ. Roy. Hort. Soc., 1901, xxvi., pp. 1—32.)

to suspend our judgment and wait for further facts. But while we wait, let us also work, and help to secure those further facts, of which we are so much in need, altogether regardless of whether they happen to confirm or not the principles laid down by Mendel.

In order to accomplish this it will be necessary to work strictly on Mendelian lines, and to study Mendel's methods with great care.

Mendel, after surveying the work of his predecessors, started with a clear conception of what he wanted to investigate, and arranged his experiments accordingly.

In his own language, he wished:

(1) To determine the number of different forms under which the offspring of hybrids appear.

(2) To arrange these forms with certainty according to their separate generations.

(3) To definitely ascertain their numerical or statistical relations.

The careful judgment, skill and forethought which Mendel displayed in organizing and carrying out his experiments with Pisum were evidently the products of a master mind, and for some time to come his classical experiments will serve as a model for the hybridist who wishes to attack the perplexing problems of inheritance.

The general object of this paper is to give a brief outline of Mendel's methods, and to endeavor to show how superior they are in all respects to the methods of his predecessors. The particular object of this paper is to express the hope that the hybridists and breeders of the New World, with their progressive ideas, their many opportunities, their vast system of experiment stations, and their practical knowledge of breeding, will take up and test the matter on a much larger scale than we can hope to do in the Old World, and thus help to bring the question to a speedy and definite issue.

So convinced is the writer of the superiority of Mendel's methods that he has already in hand a large number of experiments on Mendelian lines, in Pisum, Lathyrus, Papaver, Primula, and Paphiopedilum (Cypripedium), and also in various breeds of Fancy Poultry, the results of which he hopes to publish in due course.

MENDEL'S METHODS.

(1) *Single Characters.*

One of the most fruitful sources of confusion, in the older records of experiments in cross-breeding, has been the selection of the individual plant as the unit upon which to base the results.

The individual plant is made up of a large number of characters—organs, structures, whatever one may term them—distinctly marked off from one another, the points of difference both in form and in color being sometimes great and at other times small. In working out the inheritance of specific character in hybrid orchids in 1899, the writer became much impressed with the possibilities of variation in individuals, when a number of characters in each were considered together as one unit. (Ref. 3.)

(3) Report of the International Conference on Hybridization, London, 1899, in Journ. Roy. Hort. Soc., 1900, xxiv., pp. 106—117

Some characters showed dominance of one parent, some of the other parent, while others were intermediate.

When these several variations occurred in twenty different characters, the possibilities of variations among the individual hybrids became very considerable, so much so that the results became quite unmanageable. Since that time the writer has been compelled to consider each *single character* on its own merits. It is true that, in some cases, the correlation of characters tends to modify this result to some extent, but in the case of the orchid hybrids in question the correlation was not very evident. From this experience it follows that in any statistics of inheritance a definite result can only be determined by taking each *single character* separately as a distinct unit, completely ignoring, for the time being, the individual plant made up of many characters.

Mendel apparently was the first to see this clearly, and acted upon it in his experiments with Pisum, with remarkable results.

(2) *Constant Characters.*

Next comes the important question of ancestry. From the earliest times it has been observed that in many instances offspring have resembled their grandparents or their more remote ancestors, rather than their actual parents. So that in experimental crossing, if two parents be chosen, each of whose ancestry is unknown or perhaps consists of complicated factors, the resulting offspring are either incomparable and incomprehensible, or they vary among themselves in bewildering confusion. The result, in any case, is chaos, and goes a long way to account for the many contradictory records which we find in the experiments carried out in the old style. Mendel, in his experiments, carefully and skilfully avoids this confusion by crossing together only *constant and fixed races,* i. e., each parent has been the product of repeated self-fertilization, so that its ancestry has been practically the same for many generations.

This effectually eliminates all the possible complications which might be caused by the influence of the immediate ancestors at any rate, though how far it affects the possible reversion to more remote ancestors is difficult to say. The writer, in his experiments with orchids, has chosen *distinct species* only as parents, and in this way, perhaps, reduces the possibilities of reversion still more. De Vries, Correns, Tschermak and Bateson have all for the most part followed or carried out Mendel's method by crossing *constant races,* and it is quite possible that some of their apparent exceptions to Mendel's results may have been due to their crossing particular races which were not really so fixed and constant as they believed them to be.

As we have seen, Mendel carefully avoided this by selecting in the first instance fixed parents of pure descent; these he further tested for two years, and satisfied himself as to their perfect constancy and fixity, and side by side with his crossing experiments he was careful to carry out "control" experiments with these original parents by still further testing their constancy and fixity through all the generations.

It is just possible that these precautions of Mendel may explain the general uniformity of his results as compared with those of his disciples and some of his critics. (Ref. 4.)

(4) cf. Weldon in Biometrika, 1902, i., pp. 228—254. (For complete history, exposition and Bibliography of the Mendelian question, see Bateson's admirable hand-book on "Mendel's Principles of Heredity," Cambridge University Press, 1902.)

(3) *Differential Characters.*

The third point worthy of note in the methods of Mendel is that the characters selected for crossing must not only be single and constant, but also *differential* in the two parents. If the single characters be nearly alike in the two parents it will be impossible to determine which parent the offspring resembles in that character, because all three would necessarily be nearly alike, i. e., the offspring and its two parents. On the other hand, the wider the difference between the pair of parental characters, the more clearly defined will be the single character in the resulting offspring, and consequently the easier it will be to refer the resemblance in the offspring to either parent.

Mendel, in his experiments, takes *single constant characters in the parents which are distinctly differential* and which can be clearly defined in the offspring.

(4) *Dominant Characters.*

The fourth point in Mendel's methods is distinctly new, and that is the crossing together only of *Dominant* and *Recessive* characters, i. e., one of the characters of the differential paris is always distinctly dominant over the other one, which latter is known as the Recessive character.

This serves a useful purpose in giving uniformity in the first generation, and thus avoids the great difficulty of working on to the next generation with results which are not uniform.

For instance, if the pair of characters were of fairly equal potency, they would, on the whole, be intermediate—either blended or mosaic, tending to one parent and the other alternately. It is obvious, therefore, that in a case like this, if one wished to carry on the experiment to further generations, the lack of uniformity in the first generation would complicate the experiment so much as to make it almost unworkable.

Mendel avoids this by the selection of *Dominant and Recessive* characters only, consequently his results can easily be recorded and tabulated statistically in all the generations.

(5) *Large Numbers.*

The fifth point of note in Mendel's methods is his use of *large numbers,* and in this respect he was without doubt far in advance of his predecessors and contemporaries. In the older experiments, for the most part, only a few individuals of each cross were raised, and hence the range of variations apparent was either extreme or scarcely perceptible, according to chance and circumstances.

This no doubt accounts in some measure for the many contradictory results obtained by different experimenters at different times. Mendel avoided these difficulties by raising large numbers of individuals in each generation, and in that way practically gauged the total range of variation in each case.e

(6) *Many Generations.*

Now we come to the sixth and last method of Mendel to be noted here, and that is that he was not content to stop at the first generation or even the second, as so many of his predecessors were, but he in all cases carried on the experiments to the third and fourth generations and in some cases to the fifth and sixth generations. Mendel saw clearly that this was absolutely necessary, though at the same time the work must have been very laborious, and it

illustrates once more the thorough and painstaking methods by which Mendel overcame all obstacles in his pursuit of truth.

To sum up the methods of Mendel: Those hybridists who desire to follow in the footsteps of Mendel and his disciples and help to elucidate the baffling problems of inheritance will find it essential to select parents for the original cross which possess characters which are at once *Single* and *Constant* and *Differential* and *Dominant,* and they must also take care to raise *large numbers* of individuals in *many generations* for observation and comparison. By these methods alone will definite results be obtained.

In conclusion, as a practical illustration of Mendel's methods, a list is given of his own selection of characters in the *fixed races of Garden Peas that he used for his experiments.*

PISUM SATIVUM.

(*Fixed Races of Garden Peas.*)

Mendel's *Characters.*		*Dominant* × *Recessive.*
(1) Form of Ripe Seeds		Round × Wrinkled.
(2) Color of Cotyledons		Yellow × Green.
(3) Color of Seed-Coat	Correlated	Brown × White.
(3) Color of Flowers...	Correlated	Purple × White.
(4) Form of Ripe Pods		Inflated × Wrinkled.
(5) Color of Unripe Pods		Green × Yellow.
(6) Position of Flowers		Axial × Terminal.
(7) Length of Stem		6—7 ft. × ¾—1½ ft

H. F. Roberts: I am engaged in wheat breeding, and I should like to inquire whether (considering the fact that wheat hybridizes) one can assume in making the first hybrid that we have a succession of pure bred parents; or whether it will be necessary to make such a succession of pure bred parents, bringing them into existence by hand fertilization, in order to be certain that we have a pure bred parent. Is it safe to assume that one already has such pure bred parents?

The Chair: Will Professor Bateson kindly answer the question of Professor Roberts? It is this: Whether, in making wheat hybridization, we are to assume that we have a parentage on either side that is pure, or must we produce this ourselves by hand fertilization for a number of generations before we can confidently proceed with our anticipated results?

W. Bateson: It is difficult to speak on a subject of which I have no practical knowledge. My knowledge of wheat breeding is derived from reading the work of others, and also from some experiments that I have seen which have been conducted by Mr. Biffen, of Cambridge. Mr. Biffen holds, I believe, that the wheat is likely to hybridize, and I should imagine it is rash to infer that what one finds to be true of one variety would be true of another. I imagine that one would have, in any case of this kind, to begin by selection from any selection of plants, and then satisfy one's self that they were breeding pure and producing one form.

L. H. Bailey: Professor Spillman, who has had practical experience in that line, is here, and I should like to hear from him on this subject.

W. J. Spillman: I can only say a few words on the subject. The difficulty to which our president called attention a moment ago exists in the case of seed wheat. You may have a beardless variety of wheat, and there will be a small amount of bearded with it, which will appear in that variety every year. That is because you have that typical heterozygote; you have a small amount of it in your seed wheat, and if it is necessary to be sure it is necessary for you to grow your wheat and select the type for three generations. You may then be practically certain that you have a pure type.

H. F. Roberts: Not scientifically certain?

W. J. Spillman: Well, as near as you can ever be. In fact, you can, if you use proper methods. We can eliminate the old method of fixing a type by selection. In fact, that does not fix a type, as has been brought out here. But you can fix your type

in the third generation of any hybrid that obeys Mendel's law. If by growing your wheat, your mother plants, for three generations, you find they do come true to type, then they are true to type.

H. F. Roberts: How about mutation forms?

W. J. Spillman: I can't see that mutation forms have any particular bearing on this subject, as concerns any variety which we are considering a fixed variety as far as its being a hybrid is concerned. Maybe they are mutation forms, and I imagine that when we have come to examine a larger number of plants with reference to the mutations which are called to our attention, we will find them probably more common than we have anticipated. Yet I cannot avoid throwing out this word of caution: I may be wrong myself in it. This matter of hybridization is separate from mutation. We may have mutations in our homozygotes and we may have mutations in a hybrid plant, so that we must not confuse them. When we are dealing with hybrids let us overlook the mutations that occur. Now, there is a general belief that hybridization stimulates variation. That is a point which seriously needs investigation. I can see no reason why it should do so. For instance, Perrin, in Australia, speaks of the second generation of wheat as the variable generation. I object to the word variable in that generation, because it is not variability; it is simply splitting up in obedience to a definite, well known law, and a splitting up in a way that can be predicted.

There has been an enormous amount of work done on hybridization in the past, and Sachs, in the edition of his book published, I think, in 1879, went so far as to state that the whole question was definitely settled, and that all that could be learned had been learned then. The enormous amount of effort that has been put upon hybridization before has been an effort to discover what that heterozygote would be, what characters would be dominant, and they have been trying to determine laws by which they can predict what will be a dominant and what will be a recessive character in hybrids. I think I am not misrepresenting the facts in the case. Now we have learned that there is apparently no settling that; so that question we let alone. We wait until we get our hybrid; after we have it we can tell just what we will get ultimately, and the test of the law is our ability to prophesy by it. Now I want to repeat a statement which I have made twice before in public, that in the case of those characters which do obey Mendel's law—and the number is increasing rapidly, as Professor Bateson has said—we can absolutely state in advance, before we make a hybrid, what the result will be. That seems like a very astounding statement, but I agree with a statement of Professor Bateson already published, and which I have along with me, in which he says that he regards Mendel's discovery as of equal importance with the formation of the atomic theory in chemistry.

William Saunders: In regard to the length of time which it will take to fix a particular species, I will cite an example which we had at the experimental farm in Ottawa. We imported a wheat from near Spitzbergen, near the Lena River, which was said to be a fixed type and had been in cultivation there from time immemorial. This wheat we grew for two or three years, examining it very carefully without observing any sports of any kind in it. Of course, there might have been sports, and we might have overlooked them; but it was examined carefully for two or three years. Subsequently to that, bearded sports made their appearance in this wheat. The wheat was so small in the kernel that there was no probability of our having confused that with any other variety of wheat. We bred from those sports bearded forms of that wheat, although the beardless was the form in which we received it. It was an exceedingly early wheat, too, so that the probabilities of any confusion arising from any intermixture were less than if they had all bred at the same time.

W. J. Spillman: In that case it is possible that the bearded heads were pure sports, irrespective of any hybridization that the wheat had experienced in the past. That is possible, and is something that we cannot avoid in any way. We may select a type for an indefinite number of years, and then occasionally get a sport from it, due to something that we do not understand.

William Saunders: The selection of types for purposes of hybridization was under discussion, and this case shows the possibility of such types varying even after three years.

A paper on "Artificial Atavism," by Hugo de Vries, Director of the Botanical Gardens, Amsterdam, was read by D. T. MacDougal, of the New York Botanical Gardens.

ON ARTIFICIAL ATAVISM

By Hugo de Vries.

Crossing is a means of analyzing compound characters. It is also a means of combining the elements of such characters, and of building up the original type out of its components. In some cases the compound nature of a character may be evident, in others it is not, and in most cases it is as yet doubtful. So it is clear that a combination gained by crossing may assume the aspect of something quite new, and this will be nearly always the case where it is not possible to discern the exact relation of the "new" character to those of the parents used for the cross.

If now this "new" character happens to have been present in some of the ancestors of the crossed types, it will resemble a reversion to this lost feature, and, provided no other explanation offers itself, it will easily be taken for an example of atavism. Looking at this inference from another point of view we are led to suppose that perhaps many cases registered now as atavism caused by crossing may probably originate in this way.

In other words, we may expect in all cases, where a compound character has been lost in the course of evolution, but where its components still exist in separate species or varieties, that it will be possible to rebuild the old characters by combining the partial ones by means of crossing. Such a combination would evidently deserve the title of *artificial atavism.* Of course, I do not assert that all atavism is to be explained in this way, or that all crossing of the constituents of compound characters will have the expected result. I only think that in a number of instances the now existing difficulties may be overcome by this method.

Taking a special instance, the colors of flowers present themselves in the first rank. A great many of our garden flowers show large numbers of varities and sub-varieties that exhibit different colors. In the case of perennial plants, which are sold as plants and not as seed, it is evident that each degree in the fluctuating variability of a color may give a distinct so-called variety, as even this degree is constant enough when propagated by buds. But in the case of annuals and biennials, and even with all such species as used to be sold as seeds, this possibility disappears, and only such features as are transmitted in the course of generations may be used as good differences between the varieties of commerce. Each such variety has its own character, which remains constant and recurs in each succeeding generation, even if it is subject to much fluctuating and individual variability.

In the case of simple flower-colors which are not built up of different units the number of the color varieties is of course a limited one, and is even often reduced to the existence of a white flowered form. But if the colors are compound, and consist of two or more units, we ordinarily find a greater array of differently colored types, in so much more as the degree of composition of the original colors is greater. Besides the white variety, many blue flowers have a red form; so have many dark-red species a white and flesh-colored form (*Varieties carnea*), etc. Some very beautiful flowers have a darker color at the base of their petals, and such species often exhibit a variety in which the so-called "heart" is wanting. Other flowers are mottled, and have also spotless forms, etc. In all such cases the compound nature of the original color-type seems to offer itself as the most plausible conception and as a basis for further experiments.

If now we go through the lists of color varieties named in the commercial seed catalogues or in any descriptive horticultural work, we ordinarily find the type of the original wild species and a white variety as the two extremes of the series. Between these two we find a larger or smaller number of intermediate color forms, and we may assume that these, or some of them, are to be regarded as the elementary parts into which the original mixture may be split up. And if we apply this reasoning to these intermediate forms, we may come to two well-defined inferences, which it will be possible to test by experiment. The first of these is, that by crossing the original with the white variety the different intermediate types may be obtained. The second inference is the reverse of the first, and assumes that by crossing the intermediate types or at least by crossing the principal ones among them we could return to the original color of the wild species.

To test this argument I have chosen the common snapdragon (*Antirrhinum majus*). But before giving the detailed description of my experiments I must point out that Mendel, in his celebrated memoir on hybrids, has briefly discussed the possibility of the compound nature of the colors of most of our flowers, and has clearly formulated his conviction that by this supposition it would be possible to explain their most curious behavior in the few crossing experiments he had made with them. In fact, he says in the introduction to his paper, "Experience of artificial fertilization, such as is effected with ornamental plants in order to obtain new variations in color, has led to the experiments which will here be discussed."*

The experiments which I am now going to describe will give an entire confirmation of Mendel's predictions.**

The common snapdragon, *Antirrhinum majus,* is cultivated in our gardens under many beautifully colored varieties. The wild species or original form is of a dark red color, but there are also a white variety and many intermediate types. These varieties are constant from seed and remain pure as long as occasional crosses with other forms are excluded. The dark-red color

* A translation of Mendel's paper was published by the Royal Horticultural Society in their Journal, vol. 26, 1901, and is to be found in Bateson's work on Mendel's principles of heredity, 1902, pp. 40—95.

** For a fuller statement of my experiments with Antirrhinum and for the description of similar investigations with other species, I must refer the reader to the second volume of my "Mutationstheorie" (Leipzig, Veit & Co.), and especially to the chapter on "Die Zerlegung der Bluthenfarbe," pp. 194—206. 1902.

of the wild plant is evidently of a compound character and consists of a series of single color forms, including two elements of yellow, one confined to the underlip, which remains when the yellow is elsewhere lost in the corolla. But these yellow characters I have not included in my experiments, though, of course, their behavior in my hybrids was often observed. To determine the number of these units would necessitate a much longer study of the cultures than I was able to give them.

The red color consists essentially of two characters, a flesh-color with lively red lips, and a white or nearly white tube. These two characters are separately represented by two distinct varieties called respectively "flesh-colored" and "Delila." Both of them I found fairly constant from seed. In the following account of my experiments I will confine myself to these two elements and leave aside further analysis.

The statement here given that these two characters are the principal components of the original red color rests upon results gained by crossing this type with the white variety, and cultivating the second generation, viz., the children of the hybrids. I observed that *Antirrhinum majus* yielded a good crop of seeds when fertilized with its own pollen. It is an easy operation to castrate and fertilize them, and I always enclose the whole raceme in a bag of parchment paper to exclude the visits of insects. These bags are very effective for such experiments, and are impervious to the weather for two or three weeks or more.* I never used seed which had not been fertilized by myself.

My crossing was made in the summer of 1896. Some specimens of the white variety were castrated and pollinated with the normal red type. Next year I had a great many dark red flowering hybrids, and fertilized none of them with their own pollen. For various reasons they did not give enough seed to cultivate the next or second generation in sufficient number of individuals. Nevertheless, the splitting up of the red color in its various components was to be seen in the different lots, each obtained from the seeds of one self-fertilized plant. I give the figures for a lot of forty-nine specimens, all children of one hybrid mother, in which the separation of the different units was sharp enough to count the principal types without any difficulty. These were four, viz., red and white, the colors of the grandparents, and besides the two types named above as the components of the original dark-red color.

I counted—

Dark-red	51%
Flesh-colored	16%
Delila	31%
White	2%

A great many of these plants were self-fertilized in parchment bags, and of some of them the seeds were sown next year (1899). Of course, the seeds were gathered and sown separately for each plant.

I chose the progeny of a dark-red colored hybrid of 1898, which split up next year in the same manner as did the former generation. I had in all

* They were made by Mr. P. J. Schmitz in Dusseldorf (Germany), and are made of the same material as the bags commonly used for protecting grapes against the stings of wasps.

169 flowering children of this specimen, and found the following figures for the four principal types:

Dark-red	58%
Flesh-colored	17%
Delila	20%
White	4%

These figures evidently indicate the same law of division as the first series, but are to be assumed as a more careful representation, inasmuch as they are gained from a greater number of individuals (169 instead of 49).

The explanation of these figures is to be derived from Mendel's laws for hybrids* under the assumption of the compound character of the original color. On this assumption we have two pairs of antagonistic characters, viz.:

Flesh-colored and *white.*
Delila and *white.*

The white is evidently to be taken as the absence of both of the two opposite colors. It is not very easy to realize this condition, but yet it is clear that the white may be nothing but the absence of any color. It is a negative character, and is, if I may be allowed the comparison, simply the negation of color. But now if the color is composed of two elements, only the negative of both can lead to the white. Continuing this reasoning, I find that if we take away from the original dark-red the flesh-color character, there will remain the Delila, and if we remove the Delila the flesh-color will remain. The white as the negative of the other component will be concealed in most cases by the remaining positive character.

From this reasoning we are justified to regard the crossing of red x white as a di-hybrid crossing consisting of

Flesh-colored x *white* and *Delila* x *white*
or F x W and D x W'

and in this way we come to a proposition equal to that worked out by MENDEL for his crossings with two different pairs of antagonistic characters.

My original crossing is therefore to be represented by

White × Red
or $W+W' \times F+D$

In the first generation the negative character is recessive, as is ordinarily the case, and all the hybrids are

$W+W'+F+D.$
or $F+D$
or Dark red.

In the following generation these dark red hybrids split up according to the formula:

$$(F+W)^2 \ (D+W')^2$$
or $$(F+2FW+W) \ (D+2DW'+W')$$
which gives:

$$FD+FW'+DW+WW'+2FDW'+2FWW'+2DWW'+2FDW+4FWDW'.$$

Now if W and W' are only the negatives of F and D, the positive result is:

$$FD+F+D+W+2FD+2F+2D+2FD+4FD \text{ or,}$$

9 FD	= Dark-red	= 56.25%
3 F	= Flesh-colored	= 18.75%
3 D	= Delila	= 18.75%
1 W	= White	= 6.25%

*Ber. d. deut. bot. Ges. 18:83. 1900.

In other terms, if the dark-red color is composed of flesh-colored and Delila, and the antagonistic character of both these elements is white, the crossing of dark-red and white must give dark-red hybrids, which in the following generation must split up in four types according to the given distribution. I found, as above said:

	1st Gen.	2nd Gen.	
Dark-red	51%	58%	56.25%
Flesh-colored	16%	17%	18.75%
Delila	31%	20%	18.75%
White	2%	4%	6.25%

The accordance is such as might be expected in view of the small number of individuals counted.

If we look closer at the given combination series, we at once see that the white individuals have no other character besides this, and therefore must not be counted in their offspring. But this is not the case with the three other groups which contain partly constant types, and partly such as for one or the other character or for both are still hybrids and as such may split up again in the following generation.

The series above given leads to the following conclusion:

	Number.	Constant.	Hybrid.
Dark-red	9	1	8
Flesh-colored	3	1	2
Delila	3	1	2
White	1	1	0

In other terms, if the dark-red, the flesh-colored, and the Delila children of the first hybrids are self-fertilized, and their progeny is studied when flowering, we may expect to find some of them constant and others dividing and showing a certain amount of variation in the colors of their flowers. Only this variation will be limited as the combination series indicates. The flesh-colored hybrids do not contain the Delila characters, and therefore can only split up flesh-colored and white, according to the mono-hybrid rules. Consequently the Delila hybrids will only produce Delilas and Whites. But the dark-red may be hybrids as to both characters, or only one of them, the other being constant. In their progeny this constant character will always unite with the split-up element, and it will in this way be possible to calculate the whole composition of the progeny.

I will now return to my experiments. We have already seen that among the dark-red hybrids of the second generation some divide up in the same manner as those of the first. As I said, I fertilized a series of hybrids of this second generation with their own pollen and had the following results:

A plant of the Delila type gave 361 flowering children, all without exception of the same type as the mother, which therefore was to be regarded as constant. In the same way I had from the seed of a flesh-colored specimen 260 children, all with flesh-colored flowers only. Both of the possible constant new combinations were thus gained by crossing, and this undoubtedly is one of the chief points in Mendel's interpretations of the di-hybrids.

The figures as given above show that hybrids of the types of Delila and the flesh-colored forms may also split up in the next generation into their own color and white. According to the law of mono-hybrids, there are to be expected in these cases ¾ of colored and ¼ of white offspring. I had only two

sowings, both with the seed of a flesh-colored mother, which showed this division. One of them gave 489 individuals with 83% flesh-colored and 17% white ones. The other consisted of 156 specimens, and of them 80% were flesh-colored and 20% white.

The most interesting results are evidently to be expected when one of the two pairs of antagonistic characters has become constant while the other has remained hybrid. From the table given above two of these combinations are possible in one case, viz.:

Constant flesh-colored + Delila × white.
Constant Delila + Flesh-colored × white.

It is evident that in both of the combinations the hybrid must show the sum of the constant and the dominant characters, or flesh-colored + Delila, which gives the dark-red original type. Such hybrids cannot be distinguished either from those where both pairs are hybrid, nor from those where both the elements have become constant. All of these are dark-red, and it is therefore among the 56.25% dark-red specimens that chance must help us to work out the different types, if it is not possible to cultivate the progeny of a large enough portion of them to secure the same result directly.

When fertilizing a hybrid of the type *Constant flesh-colored + Delila × white* all the children will be flesh-colored. As to the other element they will split up into 75% Delila and 25% white. The result will be:

75% Delila + Flesh-color = Dark-red.
25% White + Flesh-color = Flesh-color.

I counted the progeny of a dark-red hybrid and found among 390 flowering children 74% dark-red and 26% flesh-colored. The mother was therefore a hybrid of the type in question.

In the same way the Delila component may be constant and the flesh-color hybrid, and the calculation gives:

75% Delila + Flesh-color = Dark-red.
25% Delila + White = Delila.

I also observed this case among my sowings, and counted among the children of the dark-red hybrid:

79% Dark-red.
21% Delila.

From these experiments it is clear that the different combinations which may be calculated by Mendel's laws, on the assumption that the color of wild species is composed of two principal elements, viz., flesh-color and Delila, are, in fact, to be met with when the individual hybrids of the second generation are self-fertilized, and the progeny or third generation is cultivated separately for each of them. This method may be called *a hybridological analysis.*

Once found, the result may be calculated by the method of hybridological synthesis, and in a more easy and direct way. With this object in view I cultivated in the summer of 1899 three specimens of the flesh-colored variety and fertilized them with the pollen of the Delila type. I cultivated their progeny in separate lots, and had 124, 142 and 187 individuals from the three mothers, or 453 in all. But all of them without exception produced red flowers, the same dark-red type as the original form. *This was the proof that this original color may be built up from its constituents, and that the varieties*

taken as such at the outset are really the true units, or at least the true principal units of the combination.

The result of this last crossing experiment may be regarded as a case of *atavism* and as the type of a long series of instances of atavism caused by crossing. To show this more clearly we must suppose that the original dark-red type had died out and was therefore unknown, or perhaps it may suffice to assume that the relation of the garden varieties to the wild form was doubtful. Not having the component characters of the red color, we would only be justified in saying that the crossing of the two varieties in question produced a new character not seen in any of them, but belonging to the wild or supposed ancestor. Evidently many crossings of cultivated varieties must in this way lead to cases of reversion.

Briefly stated, the results described here for *Antirrhinum* and controlled by experiments with the other species, we may say:

1. *It is possible to split up the colors of some flowers by crossing the colored type with the white variety.*

2. *The constituents arrived at by this splitting often follow Mendel's laws.*

3. *By crossing the appropriate constituents the original compound color may be rebuilt.*

4. *Instances of atavism may in this way be artificially produced.*

H. F. Roberts: Before raising a point suggested indirectly by this paper I would like to ask whether in making one's first cross and obtaining one's first group of hybrids, it may not be necessary (in view of the fact that the plant is a composite and all its organs and the sporophyls included are variable and differ from each other) to take all of one's pollen from a certain parent from one sporophyl? In other words, supposing you were using as a male parent a flower which is didynamous, the stamens of two lengths, is it not possible that the pollen from stamens of different lengths may differ from each other in potency; and if so, would it not be necessary to preserve one's pollen that is used in producing a certain group of hybrids from a single sporophyl in order to be within the limits of the greatest possible accuracy?

W. J. Spillman: I am not going to answer the question, but I do say that there is only one place to get the answer, and that is in his laboratory.

W. Bateson: The interest of the subject of Professor De Vries is perfectly apparent to everybody. Two points present themselves to me. I am myself engaged in trying to determine the constituents which break up a compound character into its component parts, and I am endeavoring to work out the relation of those to each other. My first comment is this: Say it is imagined that you can analyze a character into its component characters; it is obviously easy to form the converse conception, namely, that you can synthesize characters so as to rebuild a compound. But the question arises, how can it be possible to rebuild a compound character which consists of more than two components? According to the rules, the two opposite characters are contradictory to each other, and no individual can carry more than two of them. Consequently, if you decompose a compound character into more than three components, I do not see how by any scheme that we could formulate you can rebuild even those organs which shall contain those three characters. The second point is that I don't think Professor De Vries is strictly accurate in describing such rebuilding as a synthesis at all. Your purple sweet pea as you will breed it up by crossing is not a synthesis of the original sweet pea, because it will not break up into the original components which the original sweet pea contained, but it will only break up into the two components which you put into it. The original, from which the original forms were produced, bred true, but the synthesis will not. It is an apparent synthesis, but not a real one, and in my opinion the problem of synthesis is one which remains wholly unexplained.

O. F. Cook: The question of the synthesis of these characters has been raised, and that of the identity or the dissimilarity of hybrids and mutations. I simply want to

make a protest. I believe there is another way to interpret the whole class of phenomena that does not include the apparent contradictions. I don't believe that it is necessary to explain these phenomena by assuming any concrete things which are combined and separated and so on; that is, there is no reason why these are not mutations in the one case as much as in another. And I believe that it will make it reasonable to believe that the crossing of two mutations restores the parental type without the necessity of perceiving a concrete or supposedly concrete combination of characters or separation of them into aggregate units such as has been attempted by many theorists, from Darwin and Weissmann down. Of course, it is now put on the basis of characters instead of being put, as Darwin did it, on the basis of pangens, or whatever you choose to call them.

H. H. Groff: I trust that none of the plant breeders who are here to-day will for one moment attempt to place any limitations upon their effort by endeavoring to find definite results—certain clearly defined lines—as the end of their work. There are two classes of workers in this field of hybridism: One for finding out the why and wherefore, the other for the producing of results. I think I may claim Mr. Burbank, of Santa Rosa, as probably the greatest hybridist in the world of a practical character. And in that field we should not place any limitations upon the possibilities of our work. It may simplify the situation very much if we look to those greatest manifestations of the hybrid form that are available at the present moment. For example, we might take the human race, showing that there is really no possibility of limitation. The worker should look upon his work as far as possible from the position, if it were possible, of the great Creator of the universe, and assist in the advancement and improvement of the human race at the present time. Rather than place any limitations we should work to an unlimited degree for the advancement of our specialty. I have had the satisfaction of producing some 250,000 hybrids of my specialty, and the more I work the more I am satisfied that the broadest and most liberal view should be taken of our work and its future.

The Chair: I believe this statement of Mr. Groff, of Ontario, is entirely correct. We should not suppose there are any limitations; and not only not place them on ourselves, but not suppose there are any limitations upon the possibilities in this field.

A paper entitled "Some Conclusions," by Max Leichtlin, of Baden-Baden, Austria, was read by Secretary Barron.

SOME CONCLUSIONS

By Max Leichtlin, Baden-Baden.

First of all, a suitable time is necessary to have plants ready to take seed; a comparatively warm day, after a rain, with no sunshine, is best for about six-tenths of all plants; some others want a very dry atmosphere, according to the climate of their native country. No fertilization should be attempted before the stigma is ripe, a condition which after some practice is easily recognized.

The pollen to use should be looked at with the magnifying glass, and must be chosen neither too fresh nor overripe.

After fertilization it is in many cases well to put a hand glass over the flower to give it for a day or two a higher temperature than that of the surrounding air.

Pollen if gathered in good condition can be kept in small glass vessels, corked well, for several days without losing its fertilizing power. Some genera are shy seed bearers; for instance, Carygophyllaceæ, because the pollen of the flower is ripe long before the stigma is developed.

As a rule, in eight cases out of ten the female parent has the greatest influence on the form of the offspring; the male gives color. In the majority of cases the offsprings have larger flowers than either parent.

Nature in its eternal providence has put, moreover, an end to endless encroachment of man by the production of hybrids which are and remain infertile. Hybrids often become infertile, and if recrossed the offspring mostly become so. Whether fertilization is possible or not can be ascertained at once by regarding the form of the pollen grains under the microscope. If their forms are nearly alike, it will do; if the form is very different, no fertilization is possible.

The Chair: This brief paper of Max Leichtlin raises a question that has not before been referred to, and that is, how long will pollen retain its vitality? When I make any remarks I beg to say that I make no pretension to scientific exactness; but I would say that it would appear that this vitality can be preserved for several days by keeping it under a glass, which is quite contrary, I think, to ordinary experience. To illustrate: In growing vegetables in a greenhouse for family use, we find it necessary sometimes to have artificial fertilization, and for that purpose it has been my practice for many years just at this time of the year, before the first frost comes, to have a quantity of pollen gathered from the plants out of doors, plants growing in the open. This, put in an ordinary box or vessel of any sort, retains its vitality for at least six months. That is to say, all through the winter and as late as March, I have fertilized

tomatoes with pollen that has been gathered outdoors in September. We are very particular to have a crop of tomatoes early in the spring.

Then again, there are some varieties of European grapes, as the Muscat, which we grow under glass in this country, and which are also grown under glass chiefly in England, that are very poor fertilizers. Now you can take the pollen from other grapes and keep it for a considerable time to use for the fertilization of the Muscat by putting the latter cluster in a glass and blowing the pollen upon it. There are perhaps some here who can state how long that pollen retains its vitality.

S. Henshaw, called upon by the President, said: It is my experience, as that of every grape grower, that the Muscats are very deficient in pollen; they make a very poor bunch if they are not artificially fertilized. I have often gathered the pollen of the Black Hamburgh and kept it for two months in a Mason jar. Fertilization of the Muscat was attained by inserting the cluster into the jar and raising a cloud of pollen by blowing into it with a blowpipe. That has always made a perfect bunch. Leave a Muscat to its own fertilization, and you get a very poor bunch. How long the pollen would keep I am not able to say, but I have often kept it for two months.

D. G. Fairchild: The pollen is regularly kept from one year to another—I can't say just how long—to fertilize the early varieties.

William Saunders: How is that pollen kept? In glass tubes, or how?

The Chair: Mr. Henshaw states that he kept the grape pollen in an ordinary glass jar or preserving jar. He used the term "Mason jar," which is perhaps the most common form of jar in use in this country. But the question is asked by Dr. Saunders, of Canada, how can this pollen be best kept?

William Saunders: Should it be kept in closed or in open vessels, or in paper? My experience is that paper is of no value for keeping pollen. I keep it in a closed bottle, and at the same temperature.

D. G. Fairchild: I have kept pollen for a year, put away in a closed vessel in paper, and it had no effect; it had lost its vitality. It had been kept over a year.

The Chair: It is a practical question of considerable importance for both practical results and for scientific experiments.

C. W. Ward: I have had a little experience in keeping carnation pollen. I have found that put away in paper it didn't keep; but where I put it in a little vial and closely corked it kept for a considerable time.

H. F. Roberts: We also have come to the method which Mr. Ward mentioned, using little homeopathic vials with corks, being sure there are no parts of the anther mixed with the pollen grains unless they are pretty thoroughly dried. If they are greenish when they are put in, there is likely to be some moisture on the inside and some mould is apt to develop.

D. G. Fairchild: There is a very interesting piece of work in which I have been interested for a number of years, and I would ask what is the best way of shipping pollen. I have often found it difficult to secure varieties of fruits or plants for hybridizing. I am not interested in plant hybridizing myself, but have been collecting all over the world varieties for hybridizers to work with. If there is a possibility of shipping pollen, it makes it practically possible to travel through a country in the spring time and secure varieties which you could not otherwise get. It is a very important point in the work with which I am connected. Should this pollen be dried in sunlight, or is it important that it should be dried in the shade? I know that in the tropics many seeds that are dried in the sunlight are ruined. Is there any one who has had any particular experience in this matter of drying the pollen?

The Chair: The paper which is under consideration states that that depends somewhat upon the characteristics of the country in which the plant originates. That is, if it is in an arid country or semi-arid country, naturally the pollen would retain its vitality when dried in the sun; while if dried in a moist atmosphere in its natural habitat it would naturally best retain its vitality if dried in the shade.

N. E. Hansen: I have found that in the greenhouse in full sunlight pollen could not be properly dried, but the moisture is discharged, and if the pollen is put into a vial early a mould is formed. Pollen must be dried perfectly, and in the shade, for safety in using later.

H. C. Price: We had some experience in shipping pollen this last year, and have

used small pasteboard boxes. We find just the point that Professor Hansen makes: That there is danger of mould if the pollen be not perfectly dried. We use these pasteboard boxes to take up the moisture if it is present, and they have been very satisfactory in our use. I am speaking of pollen of apple, plum and cherry, and we have been sending it out from the experiment station packed in small boxes for individual use.

S. A. Beach: In my experience with the grape pollen I have found that a good method to secure a good quantity of pollen and at the same time to protect the blossoms from the visits of insects is to gather the flowers when they are about to open, transfer them to the laboratory, and have some sized paper spread over a vessel of water; punch a hole in the paper and stick the stem of the blossom into the water and cover it so as to make a moist chamber. In that way, so far as my experience goes, you can get the best development, have the best success in the opening of the anthers, and secure the largest percentage of pollen. Then after you have secured the pollen in that way, the method Professor Price mentioned is, I believe, the one to follow.

W. B. Alwood: I would like to note an experience with the pollen of wheat. I found that by going into a field in the morning after a rain, say, at night, on a bright, sunny morning at nine o'clock, and plucking the heads which seemed to be mature and holding them in my hands in the sun, I could observe the blooms open and the filaments elongate, and the pollen would be poured out very rapidly, and my idea was that in that way I could collect pollen of wheat in a little watchglass very rapidly. And I found that pollen so collected was very effective in pollenizing the female plant later, but I never kept it over one day.

H. F. Roberts: We have kept pollen for three days and found it very successful in use.

Occasionally pollen of *Amherstia nobilis,* a very handsome ornamental tree grown in the Botanic Gardens in the West Indies, has been forwarded from Trinidad to Jamaica in order to fertilize flowers in the latter island. The tree is a shy seed-bearer and the plan of obtaining pollen from other sources was adopted with good results. As far as I can remember the anthers were gathered early in the morning and placed between sheets of blotting paper and despatched by post in cardboard boxes. I should say that a similar plan might be successfully adopted with other leguminous plants when seed is not freely produced.

The following paper on "Suggestions for the Classification of Hybrids," by R. T. Lynch, Curator of the Botanic Garden, Cambridge, England, was read by the Secretary:

CLASSIFICATION OF HYBRIDS

By R. I. Lynch, Cambridge.

The most important suggestion I could make is that attention be drawn at this conference to the importance of classifying all experimental results. Investigators often want to know what plants have been found to behave in this or that particular way. They may desire to reobserve from a new point of view or to carry further the results in which they may be interested. The classification I have in view would always be valuable for reference, and would assist, I think, very largely in the determination of laws yet unknown.

What I mean is illustrated (as to hybrids) by work I have done myself in a paper on the Evolution of Plants in the journal of the Royal Horticultural Society of London, Vol. XXV. Some of my headings are very nearly as follows:

Bigeneric hybrids, fertile and true from seed.
Bigeneric hybrids, infertile.
Hybrids which come true from seed, never reverting.
Hybrids that are more fertile than either parent.
Hybrids which return, in a generation or two, to parent species.
Wild hybrids which take a position independently of either parent and are equal to "species," etc., etc.

It would be of interest, for instance, to collect examples of hybrids which at first were found nearly barren and which afterward became fertile—instances, also, of hybrids that are less fertile with either parent than with self.

In this way, I am sure, much could be done of value, in suggestiveness, for the hybridist who is concerned only in practical results.

My plan, I think, would be to ask the members of the conference to suggest all the heads they can think of—thus perhaps securing fresh ideas otherwise unexpressed—and afterward to circulate these heads (after editing) with the request that as many instances as possible should be given below them.

Writers most usually, I think, adopt a botanical classification of natural orders, as does Focke in his Pflanzen-Mischlinge, but from the trouble I have had myself in seeking out examples of this or that behavior I feel sure that much advantage would be derived by classifying in accordance with behavior itself, "infinite" though it may be in point of variety.

The Chair: Considering the great amount of work that has been done with hybrids, and also what will unquestionably yet be done, it is very important that there should be a system. This subject of classification becomes very important in this connection. Perhaps it would be well if the Secretary would read again the classification proposed by Mr. Lynch.

That part of the paper was read again by the Secretary.

The Chair: This classification appears to be based entirely upon behavior, and not upon scientific or botanical characteristics.

L. H. Bailey: I think that the first important thing for us to do is to determine upon a definition of the term hybrid. There are two notions current regarding what is a hybrid and what is not, and, of course, both ideas are correct so far as the matter of definition is concerned. There are those who think that the term hybrid should be restricted to crosses between pure species. Every one knows, of course, that this is merely arbitrary, as, in fact, true species are arbitrary. It has seemed to some (and I am one of those) that if it is important to recognize species, it is also important to recognize hybrids as crosses between species. I still believe in the righetousness of that cause, but I have come to the conclusion that we cannot hold to that distinction. The reason why we cannot hold to that distinction is a matter of usage, for all language comes to be governed finally by usage. Here is the work of Mendel coming into notice, and the work of Bateson, and of De Vries, and of others abroad, and also our own people who are making progress, as, for example, those in the Department of Agriculture. In all these works actual things are being done and actual records are being made, and not mere talk being indulged in about them, and we find the use of the word hybrid in its large sense. I am willing now to use the word hybrid in the more general sense, and then distinguish the different classes of hybrids in as many divisions as you wish—species hybrids, variety hybrids, form hybrids, and so on. Now I don't understand from Mr. Lynch's paper what is his fundamental conception of a hybrid; and that is fundamental to a classification.

W. M. Hays: It would be a good thing if we had some central place to which we could all send detailed facts as to the ease with which species and genera hybridize, that these facts might be available to those who in future want to use certain crosses to produce economic or scientific results. I feel that in many cases we go ahead and do a lot of experimenting where it is almost, or quite, an impossibility to produce hybrids. If we knew better the work of others we would not expend so much labor. We need not only to know what will hybridize and what will not, but also in many cases to know how easy or how difficult it is to hybridize certain things.

H. H. Groff: All this leads back into my former statement: That there are two classes of workers—one engaged in finding out the how and why, the other looking for the practical results. The suggestion before us is very good for those who want to find out the true inwardness of things; but, as a practical worker for final results (and in this I believe I am supported again by Mr. Burbank, who no longer keeps records of this character), I know the volume of work makes it absolutely impossible to make such records.

W. J. Spillman: I agree fully with Professor Hays that it is a matter of very great importance to the practical producer of new and valuable varieties that he should have as much knowledge as possible as to where crosses may be made and the details of the methods that must be used to secure successful crosses. There ought to be some organization by which data of this kind could be collected. For instance, if we knew the crosses that Mr. Groff had effected, it would possibly save a great many of us a great deal of useless labor, because somebody else has done that certain work. It would save us a great many disappointments. If we knew, for instance, that he had found it possible to cross two certain varieties, or two species, or two genera, it would be of great value. I want to add one remark, too, to what Professor Bailey has very well said. I don't see how we can ever use the term hybrid in its general sense, because there are hybrids that are more widely separated even than general.

H. H. Groff: Local experience and climatic conditions are to be taken into consideration. Mr. Leichtlin, of Baden-Baden, has told me that he had failed with some types both as male and female parents which, on the contrary, I had found fertile both ways. I attributed it to climatic conditions.

W. M. Hays: If we had a central place, as the Department of Agriculture, for keeping these records and having them put into accessible form, the records could easily be gotten from such men as this gentleman, who has had so much experience; not perhaps by writing, but by verbal statements compiled by men in the department. Many of those things could be compiled and in the end would be useful. As things are the records are apt to die with these men.

L. H. Bailey: Our Department of Agriculture, aside from its experimental work, is for the purpose of acting as a clearing house of experience. It might be well to take an expression of this conference before we close as to whether or no it would be useful for such records to be kept at that place.

The Chair: It is entirely proper for this conference to give expression to a matter of this kind, and doubtless it would receive due consideration from the Department of Agriculture. I understand the proposition to be that a bureau of statistics of experimental work should be maintained. After all, this is a very old thing. The world from the beginning has been doing work over and over and over again in every department of knowledge, and the general idea has been that in this experience of each investigator, and of each generation, following another, real good has come, although there has been a very great deal of waste in it. But it is hardly worth while for an investigator who is making scientific investigations, when a thing has been demonstrated conclusively, to throw his life away on that line of work. And, on the other hand, if it is found that certain things can be done in a given line, it may be important for that fact to be known to future workers.

W. M. Hays: There are here from the Department of Agriculture two gentlemen who have worked particularly along this line, and I don't know that any expression is needed. They will get the point, I am sure, and will work it up in practice.

The Chair: Perhaps the desire will be sufficiently attained by the suggestion that is made. Possibly those two members of the department who are here will give some attention to it as to its practicability.

J. B. Norton: We have been and are still carrying out in an index as full a record as we can get of published literature. As fast as it comes in, and as fast as we get the opportunity to do it, it is indexed, and in such a way that we can get at the subject and the author. In this way we are gradually accumulating a fair index of what has been published. But, of course, all that is done and not published has to remain outside of that index, except as we can get it from letters. The letters that come into the Plant Breeding Laboratory of the Department of Agriculture are indexed when anything of importance is found in them. This work has to be done in spare moments, and the spare moments of a plant breeder's time are few; and the thing is growing, particularly in the summer time, when most of us are busy at work all the time; but in the winter we have usually two or three clerks devoted to this kind of work. In that way the index is growing. Of course, on this basis it would be fairly easy to compile anything that was furnished by the different plant breeders in the country, and with the addition of one person whose time was devoted more or less to the bibliography very much of value could be added. We have not that person now on the force, but with increased appropriations that is one of the things that will come.

O. F. Cook: Mr. President, did I understand Mr. Bailey to suggest that the sense of the meeting be taken on this matter?

The Chair: The Chair understood that to be Professor Bailey's suggestion.

L. H. Bailey: I merely threw out the suggestion as to whether that was worth while. I don't know that it is necessary to put the matter in the form of a resolution.

O. F. Cook: It seems to me, Mr. President, that some formulated resolution of the meeting which was sent to the Secretary of Agriculture might at least be of some assistance in bringing about proper provision for the work which Mr. Norton has just mentioned. Some systematic index of these plant hybrids could in that way be easily kept. But if the resolution should include a recommendation to include all classes of miscellaneous information about hybrids it would, I think, be such a colossal task—possibly not at the beginning, but in the course of a few years—that the scheme would break down. An index of hybrids produced, with notes as to their parentage, could, I think, be very easily kept in the Department, and a resolution to that effect should be good.

The Chair: Would Mr. Bailey be willing to take this matter under consideration

during the conference, and if he thinks advisable prepare suitable resolutions to be acted upon at a future sitting of the conference?

L. H. Bailey: I would be willing to be one of others who should consider the matter.

It was moved my C. L. Watrous that the matter be referred to a committee of five to be appointed by the Chair, who should, if they should find it advisable, prepare resolutions to be submitted to the conference.

J. B. Norton: I wish to make a general statement in this connection. The Library of Congress has a plan of printing and publishing index cards of many subjects. They are already taking up a number of scientific matters, and Mr. Putnam, with the committee representing the American Association of Agricultural Colleges and Experiment Stations, is to print subject indices, not merely of books, but also of subjects within books, and furnish them at the mere cost of printing to any institution or library or individual desiring them. This is one of the matters that might come in under that order. The matter of the Department of Agriculture in the several bureaus and divisions taking up and doing the work of cataloguing or preparing the copy for these printed cards is being very favorably considered, and is indeed, as I understand, being started, but it looks to me as if some one ought to be especially set to the task of looking to this particular phase of literature and facts. Some of the most important points involved are not to be found by the ordinary indices to literature.

The motion was carried. The following were appointed by the Chair as members of such committee: C. L. Watrous, L. H. Bailey, W. M. Hays, N. L. Britton and H. H. Groff.

DEFINITION OF "SPORT."

The Chair: If there be any miscellaneous business to be introduced, it may be considered at this time.

D. Morris: I should like to submit this question: I notice during the discussion we are talking about hybrids and sports. In English gardening sports are limited as arising from bud variation. I don't know whether that is the acceptance of the term here. We always accept the term sport as being applied to a plant arising from a bud variation and not from seminal variation. I notice that Dr. Saunders, speaking of the wheat that he obtained, said that bearded wheat appeared after a certain time. A gentleman present regarded that as a sport. I accepted that as arising from a bud variation, because that is the only other way in which the thing could arise, since Dr. Saunders laid down the point that he had grown this wheat for some time and it was a pure stock, and bearded wheat appeared after a certain time; and a gentleman present suggested that that was a sport. We have exactly the same thing in the sugar cane. Now I have always accepted the term sport as I believe it is always accepted in English gardening, viz., as a variation arising entirely from bud variation. It is very desirable that we should have a clear understanding in regard to the use of these terms. I was entirely misled by the remark made that the appearance of the bearded wheat was regarded as the result of a sport.

W. J. Spillman: In this country, so far as I am aware, we do not understand that the term sport be confined to bud variation. Any individual that appears, either from a bud or from the seed, having a character that is apparently new, is decidedly unlike its parents in some characteristics—we call that a sport, be it from seed or from the bud. That is my understanding, at least. Isn't that true, Professor Bailey, in your usual use of the term?

L. H. Bailey: That is the way I use it, and it has been used so. I think that what Professor Morris says is true, that the English gardeners limit it as a bud variation; but in this country we call any marked variation a sport. How marked should be the variation to be a sport is a question.

D. Morris: I take it that a sport among American horticulturists is simply a form or variation, simply an indefinite form with no particular character attached to it. It is another term for variation.

L. H. Bailey: A very marked departure, a very marked variation.

D. Morris: To what degree?

L. H. Bailey: Oh, that is a matter for individual interpretation.

W. Saunders: I think that in the practice in Canada we have followed the rule mentioned by Mr. Spillman and Professor Bailey, regarding all sorts of variations as sports. Take, for instance, the Arbor Vitae. We find it separated into globose forms, and pyramidal forms, and oval forms. Some of these may be set down to one form of variation and some to the other. The difficulty we see in restricting the use of the term sport to bud variation is that it might not always be easy to determine, unless you were on the spot, whether the form had arisen in one way or the other. I can see no objection to the use of the term sport in that general way. It is really synonymous with variation.

S. Fraser: It seems to be accepted among many that any variation which you couldn't tell anything about is classed as a sport, and everybody understands it at once. It is something unexplainable, and everybody at once knows all about it.

D. Morris: In the case of Acer Negundo, you have a plant that is normal, with green leaves; a bud appears with variegated leaves. That bud is taken off and propagated, and that is called a sport. And certainly those who may use an English textbook should clearly understand that where sports are spoken of there they mean simply variations arising from the bud, and not from seed.

W. J. Spillman: Has Professor Morris any term which is applied to what we call seed sports in this country? Suppose you should plant a seed of Acer Negundo, and a plant should come from that seed with variegated leaves, have you any name for that class of variation? We call that a sport; we call both of them sports, and distinguish the one arising from the seed as a seedling sport.

D. Morris: But the very variation that you refer to as arising from the seedling may be a bud variation. It is rather uncommon, I think, for a variation of that character—that is, the variegated leaves appearing from green leaves—to arise from a seedling. I should say that where it does occur it is a sport arising from bud variation.

W. Saunders: In connection with that Acer Negundo, we have two forms of the tree, one a southern form, which is not hardy even in the western part of Ontario; and we have a northern form, which is hardy nearly up to the Mackenzie River. There is great difficulty sometimes in distinguishing between those forms. One of them, I have observed, has the leaflets usually convex, the other usually concave, but it is very difficult to distinguish between the two forms by their appearance; yet there is this marked distinction in their hardiness. Now who is to determine whether that is a form of bud variation, or whether it is a sport? It has probably come through the growing of the tree in these extreme differences in clime for a long series of years. We call that usually a "form" of the tree.

D. Morris: A geographical form.

W. Saunders: A geographical form, yes. And still it is very hard to have well defined lines to indicate all these variations.

W. M. Hays: I don't think there has been any year during the last twenty-five when I have not seen from five to fifty individuals of Acer Negundo which had leaves distinctly variegated with white and green. Now would leaves from the normal plant be called a sport in England, or would they be called a bud variation?

W. J. Spillman: We had in this country, a good many years ago, a sport, as we called it, of this character: A sheep was born with short legs like a hog. That occurred in the State of Massachusetts, a well investigated case. An attempt was made to produce a breed of sheep descended from that animal. The advantage was that they couldn't jump a fence like an ordinary sheep. The breed ran out, however, through inbreeding. Now we called that a sport, and I don't think it can be called a bud variation.

L. H. Bailey: I am afraid that this restriction of the term sport to the bud variation is a modern one. I think that this case of the sheep and analogous ones were discussed by Mr. Darwin as sports, and I think they have been discussed as sports by a large number of the evolutionary writers since that time.

The following paper by Luther Burbank was read by the Secretary:

SOME OF THE FUNDAMENTAL PRINCIPLES OF PLANT BREEDING.

Luther Burbank, Santa Rosa, Cal.

Only the most limited view of plant breeding can be given in an ordinary thesis. It would be necessary to extend the subject through many volumes to give even a general view of what has already been demonstrated, and that which the clear light of science has yet to bring forth from the depths is too extensive even for the imagination to grasp, except through a full knowledge of what practical field work has already accomplished.

The fundamental principles of plant breeding are simple, and may be stated in few words; the practical application of these principles demands the highest and most refined efforts of which the mind of man is capable, and no line of mental effort promises more for the elevation, advancement, prosperity and happiness of the whole human race.

Every plant, animal and planet occupies its place in the order of Nature by the action of two forces—the inherent constitutional life force, with all its acquired habits, the sum of which is heredity; and the numerous complicated external forces, or environment. To guide the interaction of these two forces, both of which are only different expressions of the one eternal force, is and must be the sole object of the breeder, whether of plants or animals.

When we look about us on the plants inhabiting the earth with ourselves and watch any species day by day we are unable to see any change in some of them. During a lifetime, and in some cases perhaps including the full breadth of human history, no remarkable change seems to have occurred. And yet there is not to-day one plant species which has not undergone great and, to a certain extent, constant change.

The life forces of the plant in endeavoring to harmonize and adapt the action of its acquired tendencies to its surroundings may, through many generations, slowly adapt itself to the necessities of existence, yet these same accrued forces may also produce sudden and, to one not acquainted with its past history, most surprising and unaccountable changes of character. The very existence of the higher orders of plants which now inhabit the earth has been secured to them only by their power of adaptation to crossings, for through the variations produced by the combination of numerous tendencies, individuals are produced which are better endowed to meet the prevailing conditions of life. Thus to Nature's persistence in crossing do we owe all that earth now producs in man, animals or plants, and this magnificently stupendous fact may

also be safely carried into the domains of chemistry as well, for what is common air and water but Nature's earlier efforts in that line, and our nourishing foods but the result of myriad complex chemical affinities of later date?

Natural and artificial crossing and hybridizing are among the principal remote causes of nearly all otherwise perplexing or unaccountable sports and strange modifications, and also of many of the now well established species. Variations without immediate antecedent crossing occur always and everywhere from a combination of past crossings and environments, for potential adaptations often exist through generations without becoming actual, and when we fully grasp these facts there is nothing mysterious in the sudden appearance of sports; but still further intelligent crossings produce more immediate results and of great value, not to the plant in its struggle with natural forces, but to man by conserving and guiding its life forces to supply him with food, clothing and innumerable other luxuries and necessities. Plant life is so common that one rarely stops to think how utterly dependent we are upon the quiet but magnificently powerful work which they are constantly performing for us.

It was once thought that plants varied within the so-called species but very little, and that true species never varied. We have more lately discovered that no two plants are ever exactly alike, each one having its own individuality, and that new varieties having endowments of priceless value, and even distinct new species, can be produced by the plant breeder with the same precision that machinery for locomotion and other useful purposes are produced by the mechanic.

The evolution and all the variations of plants are simply the means which they employ in adjusting themselves to external conditions: Each plant strives to adapt itself to environment with as little demand upon its forces as possible and still keep up in the race. The best endowed species and individuals win the prize, and by variation as well as persistence. The constantly varying external forces to which all life is everywhere subjected demand that the inherent internal force shall always be ready to adapt itself or perish.

The combination and interaction of these innumerable forces embraced in heredity and environment have given us all our bewildering species, none of which ever did or ever will remain constant, for the inherent life force must be pliable or outside forces will sooner or later extinguish it. Thus adaptability as well as perseverance is one of the prime virtues in plant as in human life.

Plant breeding is the intelligent application of the forces of the human mind in guiding the inherent life forces into useful directions by crossing, to make perturbations or variations of these forces, and by radically changing environments, both of which produce somewhat similar results, thus giving a broader field for selection, which again is simply the persistent application of mental force to guide and fix the perturbed forces in the desired channels.

Plant breeding is in its earliest infancy. Its possibilities and even its fundamental principles are understood by but few; in the past it has been mostly dabbling with tremendous forces which have been only partially appreciated, and has yet to approach the precision which we expect in the handling of steam or electricity, and notwithstanding the occasional sneers of the ignorant, these silent forces embodied in plant life have yet a part to play in the regeneration of the race which by comparison will dwarf into insignificance

the services which steam and electricity have so far given. Even unconscious or half-conscious plant breeding has been one of the greatest forces in the elevation of the race. The chemist, the mechanic, have, so to speak, domesticated some of the forces of Nature, but the plant breeder is now learning to guide even the creative forces into new and useful channels. This knowledge is a most priceless legacy, making clear the way for some of the greatest benefits which man has ever received from any source by the study of Nature.

A general knowledge of the relations and affinities of plants will not be a sufficient equipment for the successful plant breeder. He must be a skilful botanist and biologist, and, having a definite plan, must be able to correctly estimate the action of the two fundamental forces, inherent and external, which he would guide.

The main object of crossing genera, species or varieties is to combine various individual tendencies, thus producing a state of perturbation or partial antagonism by which these tendencies are, in later generations, dissociated and recombined in new proportions, which gives the breeder a wider field for selection; but this opens a much more difficult one, the selection and fixing of the desired new types from the mass of heterogeneous tendencies produced, for by crossing bad traits as well as good are always brought forth; the results now secured by the breeder will be in proportion to the accuracy and intensity of selection and the length of time they are applied. By these means the best of fruits, grains, nuts and flowers are capable of still further improvement in ways which to the thoughtless often seem unnecessary, irrelevant or impossible.

When we capture and domesticate the various plants the life forces are relieved from many of the hardships of an unprotected wild condition, and have more leisure, so to speak, or, in other words, more surplus force, to be guided by the hand of man under the new environments into all the useful and beautiful new forms which are constantly appearing under cultivation, crossing and selection. Some plants are very much more pliable than others, as the breeder soon learns. Plants having numerous representatives in various parts of the earth generally possess this adaptability in a much higher degree than the monotypic species, for, having been subjected to great variations of soil, climate and other influences, their continued existence has been secured only by the inherited habits which adaptation demanded; while the monotypic species, not being able to fit themselves for their surroundings without a too radically expensive change, have only continued to exist under certain special conditions. Thus two important advantages are secured to the breeder who selects from the genera having numerous species—the advantage of naturally acquired pliability, and in the numerous species to work upon by combination for still further variations.

The plant breeder, before making combinations, should with great care select the individual plants which seem best adapted to his purpose, as by this course many years of experiment and much needless expense will be avoided. The difference in the individuals which the plant breeder has to work upon are sometimes extremely slight. The ordinary unpracticed person cannot by any possibility discover the exceedingly minute variations in form, size, color, fragrance, precocity and a thousand other characters which the practiced

breeder perceives by a lightning like glance. The work is not easy, requiring an exceedingly keen perception of minute differences, great practice and extreme care in treating the organisms operated upon; and even with all the naturally acquired variations added to those secured by crossing and numerous other means, the careful accumulation of slight individual differences through many generations is imperative, after which several generations are often but not always necessary to thoroughly "fix" the desired type for all practical purposes.

The above applies to annuals, or those plants generally reproduced by seed. The breeder of plants which can be reproduced by division has great advantage, for any valuable individual variation can be multiplied to any extent desired without the extreme care necessary in fixing by linear breeding the one which must be reproduced by seed; but even in breeding perennials, the first deviations from the original form are often almost unappreciable to the perception, but by accumulating the most minute differences through many generations the deviation from the original form is often astounding. Thus by careful and intelligent breeding any peculiarity may be made permanent, and valid new species are at times produced by the art of the breeder, and there is no known limit to the improvement of plants by education, breeding and selection.

The plant breeder is an explorer into the infinite. He will have "no time to make money," and his castle, the brain, must be clear and alert in throwing aside fossil ideas and rapidly replacing them with living, throbbing thought followed by action. Then, and not till then, shall he create marvels of beauty and value in new expressions of materialized force, for everything of value must be produced by the intelligent application of the forces of Nature which are always awaiting our commands.

The vast possibilities of plant breedinig can hardly be estimated. It would not be difficult for one man to breed a new rye, wheat, barley, oats or rice which would produce one grain more to each head, or a corn which would produce an extra kernel to each ear, another potato to each plant, or an apple, plum, orange or nut to each tree. What would be the result? In five staples only in the United States alone the inexhaustible forces of Nature would produce annually without effort and without cost:

5,200,000	extra	bushels	of	corn,
15,000,000	"	"	"	wheat,
20,000,000	"	"	"	oats,
1,500,000	"	"	"	barley,
21,000,000	"	"	"	potatoes.

But these vast possibilities are not alone for one year, or for our own time or race, but are beneficent legacies for every man, woman and child who shall ever inhabit the earth. And who can estimate the elevating and refining influnces and moral value of flowers, with all their graceful forms and bewitching shades and combinations of color and exquisitely varied perfumes? These silent influences are unconsciously felt even by those who do not appreciate them consciously, and thus with better and still better fruits, nuts, grains and flowers will the earth be transformed, man's thoughts turned from the base, destructive forces into the nobler productive ones which will lift him to higher

planes of action towards that happy day when man shall offer his brother man, not bullets and bayonets, but richer grains, better fruits and fairer flowers.

Cultivation and care may help plants to do better work temporarily, but by breeding plants may be brought into existence which will do better work always in all places and for all time. Plants are to be produced which will perform their appointed work better, quicker and with the utmost precision.

Science sees better grains, nuts, fruits and vegetables all in new forms, sizes, colors and flavors, with more nutrients and less waste, and with every injurious and poisonous quality eliminated, and with power to resist sun, wind, rain, frost and destructive fungus and insect pests; fruits without stones, seeds or spines; better fiber, coffee, tea, spice, rubber, oil, paper and timber trees, and sugar, starch, color and perfume plants. Every one of these, and ten thousand more, are within the reach of the most ordinary skill in plant breeding.

Fellow plant breeders, this is our work. On us now rests one of the next great world movements; the guidance of the creative forces is in our hands.

Man is slowly learning that he, too, may guide the same forces which have been through all the ages performing this beneficent work which he sees everywhere, above, beneath and around him in the vast, teeming animal and plant life of the world.

These lines were penned among the heights of the Sierras while resting on the original material from which this planet was made. Thousands of ages have passed, and it still remains unchanged. In it no fossils or any trace of past organic life are ever found, nor could any exist, for the world creative heat was too intense. Among these dizzy heights of rock, ice-cleft, glacier-plowed and water-worn, we stand face to face with the first and latest pages of world creation, for now we see also tender and beautiful flowers adding grace of form and color to the grisly walls, and far away down the slopes stand the giant trees, oldest of all living things, embracing all of human history, but even their lives are but as a watch tick since the stars first shone on these barren rocks before the evolutive forces had so gloriously transfigured the face of our planet home.

The Chair: Anything that Luther Burbank says is entitled to most respectful consideration because of the fact that he is a man who has done so much in this field of work, the most successful worker in the field of hybridization of any in this country, and who has produced wonderful economic values from his labor.

The following paper was read by W. A. Orton:

ON THE BREEDING OF DISEASE RESISTANT VARIETIES.

W. A. Orton, Assistant Pathologist, Bureau of Plant Industry, United States Department of Agriculture.

In speaking on this subject I desire to confine myself principally to the work now being carried on by the U. S. Department of Agriculture, and will, therefore, not attempt to give any historical statements or references to other work now being done on this problem.

The experiments which are to be described were conducted in the Southern States during the past four years, mainly on the group of diseases known as "wilt diseases," or *Fusarium* diseases. The most important of these is the cotton wilt, and a brief description of it will apply very well to others of the same class affecting cowpeas, watermelons, cabbages, tomatoes and other plants, and will serve to make clear the conditions under which this plant breeding work is being done.

The cotton wilt is caused by a fungus, *Neocosmospora vasinfecta* Erw. Sm., which gains entrance through the smaller roots from the soil and grows upward through the water vessels, which it fills with its mycelium, thereby shutting off the food supply and water supply of the plant. The symptoms are usually a sudden wilting of the plant, at almost any age from youth to maturity. When the progress of the disease is less rapid there is a slow dying of the leaves, which turn yellow between the veins and dry up at the margins before falling off. The woody portion of the stem is blackened; this latter character furnishing the best means of distinguishing the disease. A microscopic examination of the stem in cross section shows the fungus filling the water vessels. The parasitism of this fungus has been proved by inoculation experiments with pure cultures. It gains entrance to the plant from the soil, where it appears to be capable of maintaining itself for an indefinite number of years in the absence of its host plant, either as a saprophyte or by means of resting spores.

In the field it appears in scattered spots, which gradually enlarge and spread and remain permanently infected. The amount of injury done varies from little to the total destruction of each successive cotton crop planted on the land. (Pl. I.) The usual remedial measures have failed to give relief. Rotation of crops is not a remedy, and extensive experiments with

fertilizers designed to stimulate and strengthen the plant, and with fungicides applied to the soil to kill the fungus, were all without effect in checking the disease.

Areas thus infected are found scattered through the Southern States, and the control of the disease is an important question, since many thousand acres are affected, and the aggregate loss amounts to hundreds of thousands of dollars annually.

The possibility of using plant breeding as a means of controlling the disease had been kept in mind from the beginning of the investigations, and after other methods had failed experiments were undertaken, with the view of discovering or originating a variety resistant to the wilt. These have proved very successful in a number of instances, which will be mentioned briefly.

1. *The Rivers Cotton.*

It was early observed that not all plants were equally attacked by the wilt disease. Frequently one of two plants in the same hill died and the other lived, while in a field where nearly everything was killed some few plants would survive and show no trace of disease. The first attempt to produce a resistant strain by selection of such plants was made on Sea Island cotton about 1895 by Mr. E. L. Rivers, of James Island, S. C. This resulted in a failure, owing to the fact that the single plant selected proved to be a hybrid without desirable commercial qualities. In 1899 Mr. Rivers, who was then co-operating with the Department of Agriculture in its work on the disease, saved seed from another resistant stalk, which he planted in 1900 in a single row through a field of his ordinary cotton. The land was badly infected with wilt, and nearly all the cotton in the field died, while not a plant in the select row was killed. This strain was planted on wilt infected land the next year, and preserved its resistance well. This season (1902) fifteen acres were planted on land which had formerly been abandoned for cotton because of its infection with the wilt fungus. With the exception of a few scattered plants, this variety resisted the disease completely (Pl. II.), while adjoining cotton of another kind, on land not so badly infected, was very much injured by wilt. These three successful tests of this selection indicate that it is as nearly resistant to wilt as any variety can be expected to be, and that the use of this or other resistant varieties will be the solution of the cotton wilt problem in the Sea Islands.

In its other features, such as length, fineness and uniformity of staple, it is above the average. It yields as much or more per acre than the ordinary non-resistant kinds, thus showing that in securing resistance to disease other desirable qualities have not been sacrificed. Its cultivation will be continued and seed will be distributed by the Department of Agriculture for the relief of the Sea Island cotton planters in Georgia and Florida.

2. *Other Resistant Selections.*

In connection with the efforts of the Department of Agriculture to produce wilt resistant varieties of cotton for distribution to the farmers in the affected districts, a number of other selections have been made, which have proved very resistant to wilt. They have now been grown for two years on wilt infected land, where they have remained healthy, while ordinary cotton

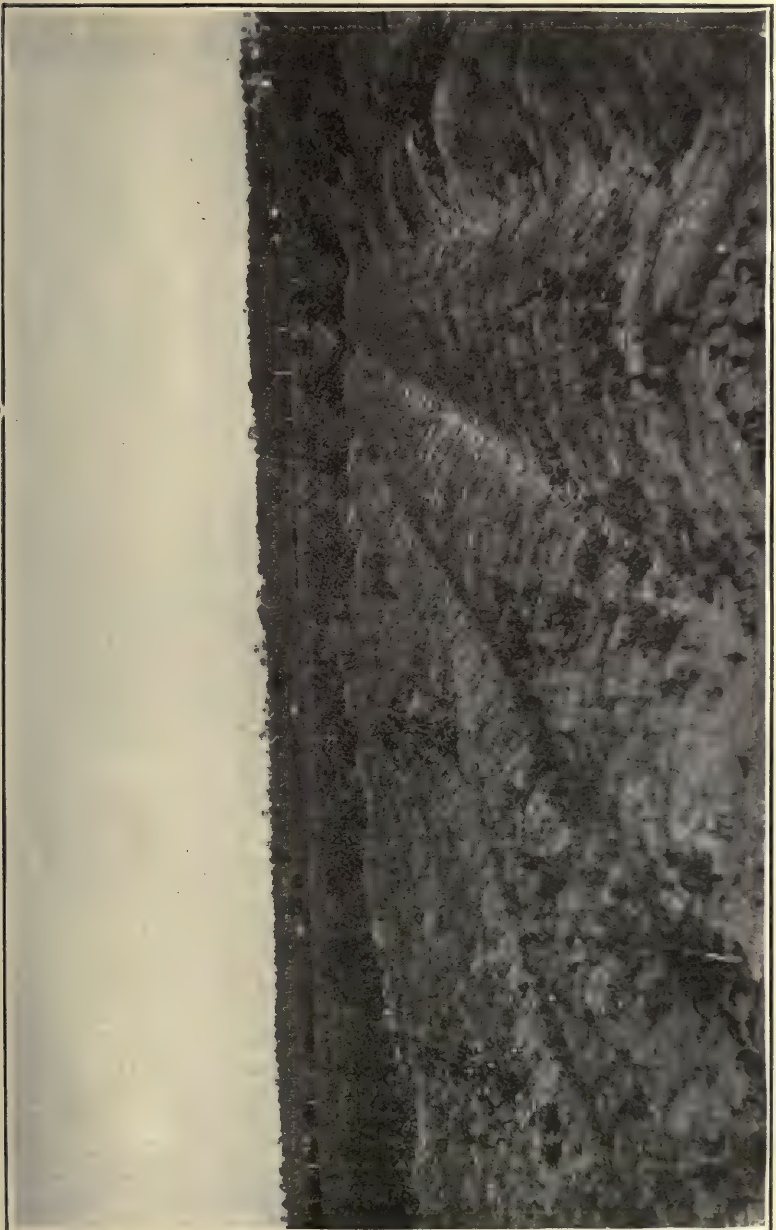

Plate I.—Sea Island Cotton Field. Nearly all Plants Destroyed by the Wilt Disease.

was greatly injured. Some of them planted beside the Rivers cotton during the present season have proved to be equally as resistant to wilt as that variety. These strains of Sea Island cotton were developed in practically the same way as the Rivers cotton—by selecting as parent stalks very vigorous and healthy plants found in the worst infected fields. The seed from each of these was picked separately and planted the next year on land known to be thoroughly infected with wilt. Under such conditions their resistance was put to a severe test. Those that failed to withstand the disease were discarded at once, and others were thrown out because they were deficient in yield or in length or quality of staple. It was found that the progeny of most of the selected plants were all resistant in the second generation, and only such were preserved. Where many of the individuals grown from one parent plant were affected the whole selection was discarded as lacking the quality of prepotency, or transmitting power. In the third generation the quality of resistance was still retained, with only a few reverting individuals showing disease. They were destroyed, and it is believed that with proper care in selection these strains can be kept resistant indefinitely.

The development of strains of Sea Island cotton resistant to wilt is so easy that it can readily be done by the planters themselves. In fact, several of the more progressive Sea Island planters are already doing this, with encouraging results, and land once abandoned because of the wilt disease is now being taken under cultivation again.

3. *The Sensation Cotton.*

An interesting instance of the origination of a wilt resistant variety is found in the history of the Sensation, a local strain of Sea Island cotton grown on Edisto Island, S. C., by Messrs. M. M. and E. M. Seabrook. The cotton planters of this section have been accustomed for many years to keep up the quality of their product by a rigid system of selection, the fundamental principle being the annual selection of a single superior plant, which becomes the parent of the strain.*

The above mentioned strain was originally selected for its length and fineness of staple, without reference to its wilt resistance, which was not observed until about the fifth generation, when it was planted in a field where some spots were known to be infected with wilt. This cotton made a healthy growth on these wilt infected areas, and thus attracted the notice of the growers. The writer made an examination of the field at this time and found evidence of the presence of the fungus in the soil in the occurrence of tufted roots on some of the resistant plants.** Later and more extended trials of the Sensation cotton have verified these observations and demonstrated the important fact that it is thoroughly resistant to wilt. The explanation of this is not difficult to find. It will be remembered that, as previously stated, there are in almost every field some plants that resist infection by the wilt fungus. The parent plant of this variety undoubtedly possessed this quality, though the field in which it grew was not infected by the wilt fungus, and all its descendants have inherited its wilt resistance. It is encouraging to note that

*For a full account of this method of selection see Webber, Yearbook U. S. Dept. of Agric., 1898, p. 358.

**See Bul. No. 27, Div. Veg. Phys. & Path. U. S. Dept. of Agric., p. 8.

Plate II.—River's Resistant Cotton on Wilt-infected Land, on same Plantation as Plate I.

this year, in the seventh generation from the parent plant, the wilt resistance is as marked as when first observed. If the other strains now being developed prove as lasting as this one their value will be very great. There seems to be no reason why we should not find them so.

4. *Wilt Resistant Upland Selections.*

So far our experiments have dealt with Sea Island cotton. The wilt disease is, however, even more destructive to the Upland varieties, *Gossypium herbaceum*, and in 1900 an effort was made to find a resistant Upland cotton. The first step was to plant on wilt infected land all the different varieties obtainable, in order to compare their resistance, and provide a place where selections could be made. The result of this experiment was to show that Egyptian cotton, *Gossypium Barbadense*, was more resistant than our Upland cotton. There was, on the whole, little difference in resistance between the common Upland sorts, with the exception of Jackson's Limbless, which surpassed all the others, though it was by no means entirely resistant. Expressed numerically, on a scale of 1,000, the average coefficient of resistance was 534 for the Egyptian varieties, 453 for Jackson and 98 for the other Upland kinds.

There were occasionally resistant plants in this field, and numerous selections of such resistant individuals were made from it, but, unfortunately, nearly all of them except the Jackson were destroyed the next year by prolonged wet weather.

It was not fully demonstrated that resistant Upland strains could be produced by selection until this year (1902), when results were obtained that were nearly as striking as those previously had with Sea Island cotton. These experiments were carried on at Troy, Alabama, on land thoroughly infected with wilt. The seed planted had been selected the previous fall from resistant plants in badly diseased fields. Some non-resistant varieties were planted beside them for comparison, and the results thus made more striking. In a number of instances single rows planted with seed from selected plants were almost free from disease, while the adjoining rows were badly attacked or almost destroyed (Pl. III.). The Jackson selections of the third generation were still resistant here, but a number of one-year selections were equally as good. These strains will be carried on and tested more thoroughly, after which the best of them will be distributed to the farmers for general cultivation.

The Cowpea Wilt.

This disease is caused by the fungus *Neocosmospora vasinfecta* var. *tracheiphila* Erw. Sm., and is similar to the cotton wilt in nature, but is not of such great economic importance. The cowpea (*Vigna sinensis*) is, however, the most valuable leguminous forage plant in the South, and any factor like this disease which hinders or restricts its use in crop rotations as a soil renovator must be considered serious. It is mentioned here merely to call attention to another striking instance of varietal resistance to disease. In this case the resistant form was not developed by selection, as were the wilt resistant cottons previously described, but was found already in cultivation, though its disease resistant qualities had not been specifically pointed out.

This cowpea, known as the Iron, has been tested in the experiments of

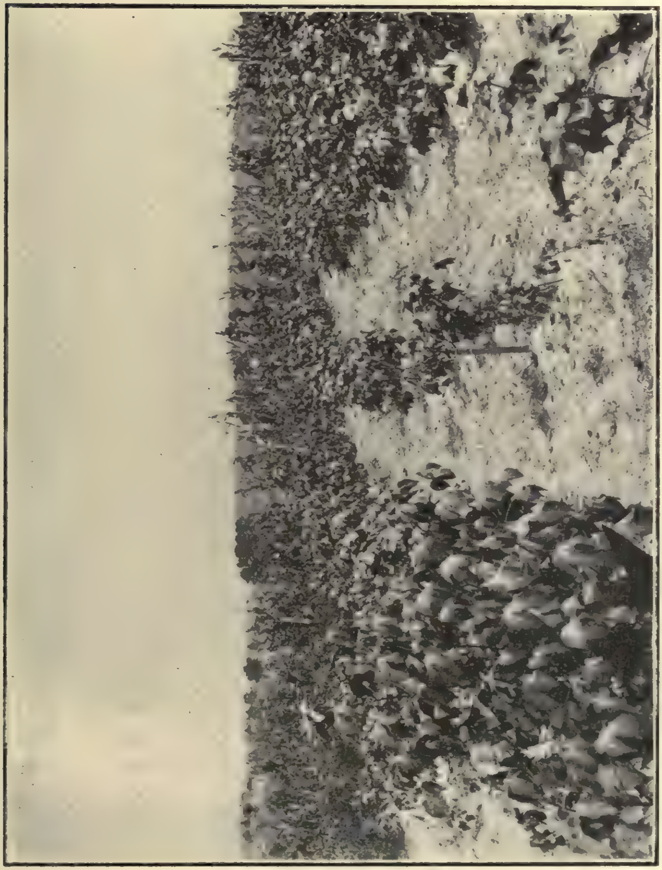

Plate III.—Wilt-resistant Cotton, Troy, Ala. Egyptian-Upland Hybrid at the left, Non-resistant Upland Cotton at the right.

the Department of Agriculture in comparison with over forty other varieties, and during the past two years has been the only kind to withstand the wilt fungus. In other varieties the loss on the infected land has been from two-thirds to the whole of the crop, while in the adjoining rows of Iron it was difficult to find a single affected plant. (Pl. IV.). Other trials of the variety in a number of places where common cowpeas had failed to succeed resulted in a satisfactory crop. The experience of farmers who have grown this cowpea on a larger scale confirms our observations. In addition to its resistance to the wilt fungus it withstands drouth remarkably well, and holds its leaves late in the season, when other varieties have become defoliated, showing in these and other respects its unusual hardiness.

The most noteworthy quality of the Iron cowpea, however, is its resistance to the root-knot worm, or nematode (*Heterodera radicicola*). This was brought out by the experiments of the Department of Agriculture a year ago,* and another year's work has given similar results on a somewhat larger scale. The Iron pea was planted beside a number of other varieties on land infested with nematodes, and careful comparisons made by examination of the roots of each lot. The Iron was uniformly free from nematode attacks and remained green and healthy till late in the season. All other kinds were much diseased and died before maturing much seed. Their roots were much knotted and deformed by the nematode galls, while the roots of Iron were uniformly smooth and clean.

This attribute of the Iron cowpea, if maintained in other localities and through later years, will make it of very great value as a rotation crop throughout the wide areas in the Southern States where nematodes are prevalent. In these sections the use of the common cowpeas is often injurious because of their extreme susceptibility to root-knot, since a crop of them will so greatly increase the number of the parasites in the soil that succeeding cotton or other crops will be injured more severely. No other leguminous crop equals the cowpea as a soil renovator in the Southern States, and if this variety can be safely used on nematode infested land it will have a wide range of usefulness.

Another point of interest to us as plant breeders in connection with the resistance of this variety to nematode attacks lies in the encouragement it gives us in our efforts to produce nematode resistant strains of other plants. If such forms of peaches, cotton, tomatoes and other vegetables can be produced they will be extremely valuable, not only in America, but also in other countries where nematodes cause injury.

The Watermelon Wilt.

This disease, caused by the fungus *Neocosmospora vasinfecta* var. *nivea* Erw. Sm., is similar in nature to the two preceding, but is more widespread in our southern and central States. Its action is also more rapid and the destruction it causes more complete. In sections where it is prevalent seven to ten years must elapse after land has been planted to watermelons before the same crop can with safety be put there again. We have had some diffi-

*Webber, H. J., and Orton, W. A.—Some Diseases of the Cowpea, Bul. 17, pp. 23-38. Bureau of Plant Industry, U. S. Dept. of Agric., Washington, D. C., 1902.

Plate IV.—Experimental Cowpea Field. Wonderful Cowpea at the left. Iron Cowpea at the right, showing resistance of the latter to Wilt and Rootknot.

culty in obtaining wilt resistant selections of the watermelon, because resistant individuals are less numerous; but several strains are now being grown which promise to retain this quality. Selections were made this year from resistant vines which remained healthy on land where all melons had been killed for four years previous. Our hopes are further stimulated by the fact that the citron and an African melon, both forms of *Citrullus vulgaris,* are resistant to wilt. Hybrids between these and our better watermelons have been made, and they promise to be wilt resistant, though it is not yet known whether an edible product can be obtained. This work is now being continued.

Discussion of Methods and Results.

The question of the reason for the marked resistance to infection by wilt fungi noted in individual plants of cotton, cowpeas and watermelons has not been satisfactorily answered. Our own observations lead us to believe that it is not due to any mechanical protection afforded by thicker cell walls or in similar ways, but that it probably is due to some physiological reaction within the plant, possibly connected with the presence or absence of certain enzymes. This interesting problem has not yet been fully worked out. It is certain that resistance to one disease does not necessarily imply any immunity against others. One of the wilt resistant cottons, for instance, is very much affected by the angular leaf spot.

The important points that concern the plant breeder are that individual plants do occur which resist these wilt diseases, and that their descendants inherit this resistance. The determination of the proper methods to pursue in utilizing these variations and fixing them into stable varieties has been the object of some of our investigations. The final conclusions can hardly be drawn as yet. They will probably be found to confirm principles already known to plant breeders.

Importance of the Individual.

The most important point that has been brought out is that in seeking a disease resistant variety the attention must be directed to the selection of resistant *individuals* to use as the parents of a new strain, rather than to the discovery of the desired characters in a variety already existing. There is no question, of course, that varieties differ in their susceptibility to disease, and a thorough test of a large number of varieties in a location where they will be exposed to the disease against which resistance is desired would be the first step in a system of plant breeding. But if even the best variety is not entirely resistant, the selection from it of individual plants that have the desired character may lead to the production of a resistant strain.

Furthermore, it is very desirable to save the seed from each plant separately, that is, to take as the basis of each selection a *single* exceptional individual, rather than to mix the seeds of a number of good plants. The reasons for this are, first, that it insures greater uniformity than can be had from seed of mixed parentage, which is especially desirable in breeding cotton; and second, that if any selection lacks prepotency or shows other undesirable characters, it can be wholly discarded in the second generation, while in mixed lots this could not be done; third, opportunity may be had for exact studies on heredity. If the aim is to produce a select variety too

much emphasis cannot be laid on the desirability of concentrating one's energy on the careful comparison and selection of a few mother plants, which are to be kept separate, rather than on the "rogueing" of large fields.

The Factor of Cross-Pollination.

When the selection of wilt resistant strains of cotton was first undertaken considerable importance was attached to the close fertilization of the flowers on the selected plant, since it would seem that if they received pollen from adjoining non-resistant plants the seed produced would be less resistant. Our field experiments have not supported this view, however, and at present little importance is attached to the covering of selected plants to protect them from foreign pollen, though as a precautionary measure the plants are isolated as much as possible, in order to reduce the amount of cross-pollination. In our experiments a large number of flowers were covered with bags and allowed to fertilize themselves. The seed thus obtained was planted on wilt infected land besides some from open fertilized bolls. The results showed no advantage from self-pollination either in uniformity of type or resistance to disease. The plants produced from uncovered flowers were, if anything, more vigorous than the others. It should be said in this connection that it is probable that a considerable percentage of cotton blossoms in the open field are normally close fertilized.

Fixing the Type.

It was thought at the beginning of the work that the selection for resistance would have to be continued for two or three generations in order to fix the type. Experience has shown, however, that with cotton our resistant types are fully fixed from the beginning. In our present work, if the offspring of any selected plant fail to show this uniform resistance, they are all discarded. Such cases are few, and may be due to an error in selecting as resistant a plant never exposed to infection, or the plants may have lacked prepotency.

Effect of Fertilization.

In our work on the wilt diseases it has been found important to have the land where the selections are to be made well fertilized, particularly with stable manure or other organic substances. The resistant plants attain a better development under such circumstances, and it is easier to distinguish between resistant and partially diseased plants.

Selection vs. Hybridization.

In breeding wilt resistant varieties of cotton it has been found possible to attain the end desired by selection alone, without having recourse to hybridization. Hybrids have been made in connection with the work, particularly with Egyptian cotton, and have proved very resistant (Pl. III.), but the difficulty of securing a fixed type of commercial value is so great that we have not preserved them, since selections from well fixed varieties answered the purpose better. With watermelons the same objection to hybridization holds with greater force on account of the difficulty usually experienced in fixing hybrid cucurbits. In working with other crops and other diseases it may be desirable to use hybrids, but under our conditions

it seems to be possible to obtain by selection alone disease resistant strains of any quality desired, whether fine or coarse, late or early.

Selection for Resistance to Other Diseases.

This comparatively unworked field in plant breeding offers great rewards to coming investigators. There is every reason to think that strains of plants can be developed which will resist many of our common and destructive diseases. Many instances could be cited where the results of such work appear promising. The production of disease resistant fruits, nuts and potatoes may be looked for; but it should be remembered that results obtained with varieties propagated by seed, like cotton, may not be duplicated with equal ease in varieties propagated by grafting or buds.

W. M. Hays: Is cotton generally open to cross fertilization?

W. A. Orton: That is an uncertain point. Cotton fertilizes itself freely; that is, flowers covered with a bag before opening are almost certain to set seed, and yet they are freely visited by insects, and there is no question that abundant cross fertilization takes place. But our results have been contradictory. Where different varieties have been planted beside each other there has apparently been little crossing in some cases, but in others a great deal. I think that cotton as a rule is not extensively crossed by insects in the field.

W. M. Hays: Have you used hybrids in large numbers; in anything like as large numbers as your standard stocks?

W. A. Orton: Hardly; hybrid cottons are very difficult to handle on account of extreme variation.

The Chair: Have there been any experiments toward getting a variety of muskmelon or cantaloupe resistant to wilt disease? That is a subject that appeals to all of us.

W. A. Orton: The wilt of the muskmelon is due in the North usually to a form of black rot. In the South there is a wilt of the cantaloupe which is due to a disease very like the watermelon wilt, and I am working on that. So far as I know, no experiments have been made on the Northern disease.

W. M. Hays: I would like to ask the speaker if he knows of what this resistant quality consists.

W. A. Orton: The Department has just found that there are no anatomical features that would explain the difference; and I believe the resistance is due to physiological or chemical differences in the plant. The fungus seems to be unable to enter the roots, or if it does obtain an entrance to the smaller roots it is unable to penetrate the water passages, and I believe, although I have no definite proof of it, that the reason for the difference is an enzyme of the plant.

D. G. Fairchild: In 1899 I visited the experiment stations in Svalof, Sweeden, and Dr. Nilsson called my attention to a patch of vetch which had been selected from a single resistant plant which has resisted the attacks of Peronospora. The whole field had been killed by this disease with the exception of this one plant. They had selected it and raised plants from it and produced as beautiful a patch of vetch as I have ever seen. My attention was recalled to this by Dr. Orton's remark that he believed there was no limit to the possibilities of selection in plants for resistance to disease.

H. H. Groff: Not long ago articles went the rounds of the horticultural press which gave the impression that since all seed was produced free from disease, we might sow with the expectation of securing strong and vigorous plants from the seed of diseased parents. This, however, proves most conclusively, I think, that that must be an error. Cotton is produced from seeds, is it not, Professor Orton?

W. A. Orton: Well, in the case of these diseases, seed saved from diseased plants is certain to become diseased if grown in infected soil. In a different kind of soil not affected by this fungus, it is likely to grow and remain healthy, although, perhaps, not as vigorous.

S. Fraser: Is it necessary to grow the crop on diseased land in order to secure immunity?

W. A. Orton: It is necessary to grow crops on infected land in order to make selections.

S. Fraser: To maintain the immunity? W. A. Orton: I don't think so.

S. Fraser: If grown on other free land for a series of years will it then be resistant?

W. A. Orton: Except in this way, that if planted on infected land the non-resistant plants can be weeded out, while if grown on non-infected land they would not be noticed.

S. Fraser: Could you mention any variety of potatoes that is resistant?

W. A. Orton: I could not just now. I have that information in my notes, but I don't recall any variety resistant to ground rot.

The Chair: The subject treated by Professor Orton is one of very great economic importance, and we can only hope that the experiments will go on and very effective results be obtained. I am sure that there are opportunities in many fields for experimentation in this line of selection, and it is something that we can almost any of us do and do it with a hope, after hearing this paper, of satisfactory result.

The following paper was read by W. M. Hays:

BREEDING FOR INTRINSIC QUALITIES.

W. M. Hays, Agriculturist, State Experiment Station, St. Anthony Park, Minn.

The aggregate of wealth annually produced in America by means ot domestic plants and animals is valued at several billion dollars. A liberal allowance for fields and flocks in the hands of men who will not seek or even receive blood of improved plants and animals will easily leave annual products worth two billion dollars, which may be brought under the breeder's influence. Burbank and other breeders of plants, and of animals, have abundantly proved that it is practicable by breeding alone to increase values ten per cent. Ten per cent of two billion dollars is two hundred million dollars. No doubt, breeding operations, as now developed, will increase this annual product two per cent in twenty years, or forty million dollars annually. It is fair to believe that an additional increase of eight per cent, or one hundred and sixty millions additional annual increase, may be produced by devising and extensively using much better scientific methods of breeding. One per cent of the total increase should be a sufficient annual expenditure to produce the magnificent increase which seems in reach, if the work is properly carried out. The soil is a vast mine, and our plants and animals are parts of our mining machinery. If by spending one per cent of the value of the output on these machines we can increase the output ten per cent, we are "penny wise and pound foolish" not to do so.

This problem is distributed from the gentle violet to King Corn, and from the brilliant goldfish to the queenly cow, and includes many microscopic organizations. On the side of wealth production this problem lies mainly in a score of staple crops, and in the four great species of domestic animals. But in addition there are vast financial interests in species of fruits and flowers and in the minor species of economic animals, including some fishes, oysters; also economic insects, bacteria, etc. The interest does not stop with the financial, but there are ethical and scientific interests of vast import. Improved plants and animals, and the interest which is connected with their breeding and production, make for a better civilization.

Pedigrees, distinguishing marks, performance records and the attractive presence of the living product of the breeder's art have an inspiring interest which is uplifting. People who have highly bred plants and animals farm better, thrive better, enjoy life better and live better. Breeders and growers of improved plants and animals have a delightful source of zest, unfelt by ordinary mortals. Humanity relishes touch with improvement. The hybridizer is a

creator of values, and his every improved product, besides serving in its own generation, becomes a new foundation for an added story to the superstructure of economic or artistic production.

Man's influence on plants and animals was Darwin's best clue to the science of heredity. Hereafter scientific breeding promises to be one of our best means for studying the nature of man, as well as of plants and animals. Man, aided by social organization, is making tremendous strides in self-development. The rapid strides found possible in improving species of plants and animals begin to give hope that race development may become feasible. The subject of breeding is apparently approaching a vastness, a profundity and a widespread interest comparable with its possibilities.

As minds flock about a new thought in invention, literature or art, and perfect it, so scientists have long hovered, spellbound, about Darwin's theory of natural evolution. That man can turn this idea to immense account has heretofore received only a moiety of its due. Though the strenuous contest which his theories precipitated caused him to lay the stress on the historical rather than on the future economic phase of his brilliant work, as relates to animals and plants, Darwin saw both facts as few of his followers have appreciated. But there is developing a deeper appreciation of his work in relation to the possible evolution of domesticated species. Biologists, as well as practical breeders, are becoming interested in the possibilities of artificial evolution, carried out scientifically and on a scale commensurate with its necessities and importance.

The most important business principle brought out during recent years by Burbank and others is that by carefully growing and testing very large numbers of plants improvements may be made which will far more than pay the cost. Galton showed that there is only one remarkable man in several thousands. Animal breeders have come to realize that there is only one remarkably prepotent and effective sire in many; *e. g.*, Messenger, the father of trotters, and Stoke Pogis, sire of a family of superior Jersey cows. Gideon, Patton and other apple breeders find only one superior apple among very many seedlings. Burbank throws away piles of berry vines as large as straw stacks in the effort to find a few superior individual mother plants.

The old time proposition that like begets like, that there are variations in some individuals, with occasional cases of atavism, is neither distinct nor definite as a rule of business action. Breed from the best is a good rule, but its meaning is comparatively unimportant in variety and breed formation, unless changed to read: Breed from the very best from among very large numbers. To utilize large numbers in an effort to secure the one or several best as progenitors of a new race, system must be used in growing, measuring and testing. Statistical methods, instruments of precision, and machinery, also business and public organization, must be devised and utilized extensively, especially with some of the most important wealth producing species.

Selection requires nearly the whole labor and expense, in most cases hybridizing only requiring a fraction of one per cent of the work of the improver of plants and animals. Plants may be divided roughly as to methods of breeding into three classes, according to their manner of propagation. The

apple will serve as an example of the first class, where we reproduce the mother plant asexually, as by buds or cuttings, there being only bud variation and no further adulteration through fertilization. Wheat, which is self-pollenated, but which we reproduce by sexually produced seeds, will serve to illustrate the second method of breeding. Corn, which is accustomed to the freest and widest cross-pollenation, will serve to illustrate the third, and by far the most difficult general method of breeding. I use the term "breeding" in its general or widest sense, so as to include all operations which assist in securing blood lines better adapted to the desired purpose.

The first operation is securing the best foundation stocks, whether utilizing varieties at hand known to be superior, or by accession from the outside, securing others and testing them beside known standard sorts. We should always place much effort on securing the best possible start. Of the possible final improvements more than half is ofttimes secured by a wise choice of foundation stocks. Not infrequently ultimate failure is met because of a hasty and ill advised start. Breeders are in competition, and no one should allow his competitors to start with superior foundation stock and he with a handicap. This selection of original varieties should be from among as large numbers as practicable, and with a broad knowledge of the correlated qualities required to make up the largest unit of economic and artistic values in the desired line. As the methods of selecting are here much the same as selecting among newly formed varieties, details will be passed over.

In making hybrid varieties the mere manipulations of emasculating and cross-pollenating, though sometimes tedious, are usually exceedingly simple, and in many cases may best be left to natural agencies. The choice of varieties to mate, however, presents unusual difficulties. In some cases, *e. g.*, Burbank's plums, experience has proven that the union of certain blood lines often results in an unusual proportion of superior progeny; and in animal breeding certain out-crosses have so often proved superior that they have gained popularity. But for the most part crosses must be made between those varieties which most nearly approach the desired ideal, and which will supplement each other; and then the chances must be taken of securing occasionally superior parent plants and effective blood lines. True, in some cases, we can gradually introduce a small proportion of the attenuated blood of some form strong in a needed characteristic, but undesirable in others. Thus the thirty-second part of the blood of a hardy native crab might be utilized to make a hardier form of apple for the far Northwest. Since hybrids may combine their innumerable characteristics in such a multitude of forms it is not strange that we find it necessary to seek from among tens and hundreds of thousands *that one individual or small group of plants* which shall possess the desired correlation of qualities. If all the words of a dictionary were on slips of paper and placed indiscriminately in a pile, we could be certain of securing a given word only by looking over the whole pile. The combinations possible in hybrid individuals between two species or varieties are vastly more numerous than the number of words it is possible to make by combining the twenty-six letters of our alphabet; and where the blood of three or more species is intermingled the complexity is made still greater. We must expect to select from among large numbers.

Varieties Multiplied Asexually.

In species like the apple the opportunities of improvement by selection within the variety, without seed reproduction, are relatively slight, because confined to bud selection; though even in buds the natural impulses of the mixed blood, aided by environment, cause useful and valuable variations, apparently differing in their values only in degree from variations arising directly from seed reproduction. Seeds which are hybrids between known varieties should generally be used where practicable. Here the selection is carried out by simply growing immense numbers, digging out all unpromising trees, fruiting the promising ones, and by making root or top and bud grafts, thus extensively testing the very few best, and finally propagating only those which prove so superior as to warrant their commercial use. If better methods cannot be devised for securing to originators remuneration for their work, the breeding of this class of crops should be taken in hand extensively at public expense. In Minnesota something is being accomplished by the State Horticultural Society. It has offered a premium of $1,000.00 for a choice apple, with certain prescribed qualities. But that sum can pay for only one variety, and even that liberal premium will not give enough stimulus of apple breeding. The public cannot afford to longer refrain from investing much larger sums in apple breeding.

Varieties Multiplied by Self-Pollenation.

Any reasonable expense may properly be used in the breeding of our great staple crops, where hundreds of millions of dollars hang on the breeding values of a comparatively small number of superior plants. Theory dictates and practical experience has proven that the employment of a large number of plants is an economical necessity in breeding such crops as wheat, oats, barley, etc. The following brief statement of a plan for breeding wheat is essentially that in operation at the Minnesota Experiment Station, but is adapted to the conditions of starting anew in a State experiment station:

A large number of chosen varieties, a hundred or more, are secured from regions with conditions most like that for which the varieties are desired. In a few years these are reduced to the ten, more or less, which are productive of the largest value per acre. The breeding may be begun before all these varieties have been given field trials, if one or more varieties are already at hand which are useful, that they may be made more useful to the State at the earliest moment.

For convenience of statement, after testing three to five years, we will assume to choose five superior varieties for foundation breeding stocks. At every point where selection is practicable the best should be chosen. From field plots of each of the five varieties superior spikes are chosen. These are shelled and 10,000, more or less, superior kernels are selected from each variety. Each of these is planted by itself in a bed with its fellows in such manner that each plant may have the same opportunity as each other plant. Beds four by forty-two feet, with two-foot alleys between the sides and six-foot alleys between the blocks, at the ends of the plots, are made level and the seed bed made fine and of very even texture. To insure good mechanical conditions and an abundance of moisture in the spring, also free-

dom from insects, the land for the field crop nursery is summer fallowed the previous year, where practicable to do so. An effective machine has been devised for this planting. It rests on planks lying in the alleys on either side of the bed. One man sits on the machine and drops into each of fourteen cups one or two seeds. He then throws the frame carrying the cups over and the seeds fall down fourteen tubes, each of which extends into the soil to a uniform depth of two to three inches. A second man with a lever throws the machine forward four inches, and as the seeding tubes are four inches apart the hills are in squares, each plant having sixteen square inches of soil room. Before the plants have begun to stool the beds are carefully gone over, and only one plant is left in a hill.

When ripe the poorer plants are cut off with sheep shears, leaving about 500 of those of each variety, or 2,500 in all, which appear to be the heaviest yielders. The bunch of several spikes of each of these plants is put into a separate packet, properly numbered, and taken to the laboratory and weighed in the chaff. One hundred of each variety, 500 plants in all, which weigh heavy, are shelled and inspected as to quality. Two hundred or more of those yielding the highest value per plant are chosen, and the second year 100 of the best seeds are planted from each of the 200 mother plants. Two border rows are planted about each plot, and these are removed before harvest. At harvest time ten superior spikes are saved from as many superior plants in each plot, and the seeds from these are saved to plant a similar plot the third year from each stock or variety from the 200 mother plants, respectively. There are usually a few blank hills, and it is necessary to count the plants as harvested. Shears with automatic counting attachment are here needed to facilitate the work and to make the counting more accurate. The grain is tied into a bundle, the heads of which are wrapped in muslin to prevent loss from sparrows, etc., and the bundles are tied to stakes to dry. The bundles are threshed in especially arranged machinery, which does not lose nor mix the grain. After adding the weights of the bulk grain and that shelled from the ten spikes chosen for seed the total is divided by the number of plants, thus giving the yield in grains per plant of the progeny of the 200 mother plants. By using the seeds from the ten spikes a similar test is made the third year, and in like manner a test is sometimes made the fourth year. The averages between the yields of the average plants for the two or three years give pretty good indexes to the power of the blood of each of the mother plants to produce heavy yielding progeny under conditions nearly like those met by the wheat plants in the fields. In other words, we thus secure a good measure of the "projected efficiency," or breeding power, of each of the 200 mother plants. The seeds of each of the fifty best of the 200 stocks may now be separately planted in field plots, making fifty varieties; or, five, more or less, of the plants very similar in appearance may be mixed together, thus greatly reducing the number of varieties. These are grown the fifth year in increase plots, so as to secure sufficient seeds for plots of one-tenth or one-twentieth of an acre in area. The sixth, seventh and eighth years the fifty varieties are grown beside their parent varieties and other standard kinds in field test plots. Sufficient superior stocks are handpicked out of each plot to furnish seed for planting the plot the next year,

thus keeping the plot pure from mechanical mixture and gaining slightly on the selection toward larger yield. During these three years tests are made not only of yield and of quality, as determined by inspection, but milling tests are also made of the quality of the flour from each variety. At the end of this period averages are made of the trials for the three years, and each wheat is studied as to its relative value—producing power per acre. Possibly five out of the fifty are superior to the remainder. These are furnished to experiment stations in surrounding States, which are in a co-operative organization, that they may give the new sorts further trial for the use of the originating station, and also for their own use. If, after one or two years' trials at several stations, or after further trial at the originating station, one of these wheats shows a value per acre greater than any other wheat grown, it is rapidly multiplied. Instead of distributing it free, the pedigree is placed behind this new variety, and it is sold at a price much above the current prices of ordinary seed wheat, but much below its real value. It is sold to seedsmen, seed growers and farmers, and so distributed throughout the State that all who will make a specialty of growing superior seeds for sale may soon have a chance to grow it. This has proven a very effective way of widely distributing a variety. Even sorts bred only for superior yield per acre, with no ordinary varietal distinguishing marks, are thus widely distributed.

Distinguishing marks are very useful, but, often to combine breeding for distinguishing marks with breeding for intrinsic qualities has defeated the whole economic purposes, *e. g.*, breeding clover at the Minnesota Station for the first ten years of effort in that direction helped to lead to temporary failure. We tried to breed hardy clover with flowers of lighter color.

This method of breeding wheat costs money. "The proof of the pudding is in the eating," however, and one variety thus originated and distributed, Minn. No. 163, has already paid the bill for breeding this and other species of field crops by the Minnesota Station. The farmers' reports show that this variety yields a dollar per acre more than the wheats it is supplanting. We estimate that in 1902 there was grown in Minnesota 60,000 acres of this variety; also 20,000 acres in North Dakota. That this has resulted from 200 bushels distributed in 1899, 100 in 1900 and 200 in 1901 seems at first remarkable. This emphasizes the fact that even a variety not differing in appearance from a kind commonly grown in the State may be widely distributed by putting behind it a statistical pedigree and by careful work in breeding and distributing which will gain the confidence of the growers.

Open-pollenated plants, also animals, must be bred in many ways in a manner radically different than bacteria, or apples, or than wheat or oats, where adulteration by mixing is not continued. Open-pollenated species and animals are accustomed to more or less cross-breeding, and in many cases a mixture of blood is necessary to their reproductive vigor. Here we have the more complex problem of securing *mixtures of blood lines which have a high "projected efficiency."* It is harder here to fix types, to attain a settled character of blood lines—which are constantly tending to reorganize into new combinations of characters—so as to have a strong and constant efficiency in the production of large values along given desired lines. Besides, we cannot

breed a large number of open-pollenated varieties in one nursery, nor on one farm, unless it be a very large one. In corn, for example, the importance of great care in securing a superior foundation stock is even more important than in wheat, because the number which may be kept under trial is limited to so few. After having chosen the variety, the very choicest plants should be chosen from among very large numbers, as from large fields. These should be most carefully tested as to yield of grain and general character of plant, time required to mature, etc. In some cases the composition of the kernels may be determined. One hundred or more superior plants are chosen, and a centgener row planted from each. Twenty or more of the best yielding rows with good plants are chosen, and from among the plants in each of these rows ten or more of the best plants are chosen and tested. The best of these are used for planting thirty to a hundred centgener rows the third season. Care is taken to not reduce the number of original blood lines to below, say, five, or even ten. Soon the selecting should begin to respect certain types, that the variety may be reduced to a more popular appearance. But retaining the high yielding types is of first importance, and a check should constantly be kept on the work. There is some danger of making the variety uniform, or "thoroughbred," in appearance and mongrel in yield. Breeders of Shorthorn cattle sometimes find that they have "thoroughbred red" families which are "scrub milkers" and not very thoroughbred—uniform—in beefing qualities. Once the value per acre of the variety of corn has been materially increased, the very choicest ears only being needed for the nursery breeding, seeds from the best remaining ears may be used in field planting, and seed grown from this selected stock for the market. The nursery must be large to give wide opportunity for securing superior individual plants and for testing the breeding power of many mother plants. Every year, or every few years, a commercial stock of seed may be drawn from the nursery, taken to the field and there multiplied. It may pay to grow corn in the nursery only every alternate year, growing in alternate years in large fields, for which purpose the seeds of the various blood lines are indiscriminately mixed. No one can settle this and many other similar questions until experiments are made.

Just now there is an excitement about Mendel's Law. We hope that law will help to clear up many questions, both scientific and practical. But there are thousands of theoretical questions with practical bearing. Some of these may be as important, or even far more important, than Mendel's Law. Let me urge that we extend, broaden out and intensify our endeavors in investigation along these lines. Let us urge the large equipment of laboratories and experimental grounds for plant breeding at public expense. Let us find and introduce those laws of business which will enable the private individual to secure larger rewards for supplying the world with new creations. The scientific workers in plant and animal improvement should realize that the people will appreciate large economic results, and will gladly urge the expenditure of what appears now very large sums of money, if we show as large a dividend as we might. Creating values and scientific work go hand in hand. The production of new wealth will supply the wherewithal to support scientific research in natural and scientific evolution. Scientific research,

on the other hand, will furnish the laws to make easy and popular the production of wealth by dealing with the living forms we depend upon to produce such a large part of the commodities needed by the world.

The most important law is that by hybridizing we can create new values. The next most important law is that by selecting from among very large numbers we can segregate the individual plants or animals carrying these new powers and multiply them for our use. The great business proposition is that values can be enormously increased at relatively slight expense. Are not the government, the States and private individuals ready for larger co-operation, not only to learn more about how to breed, but also to extensively improve our useful friends, the plants and animals, upon whose powers of inheritance we so fully depend?

The Chair: I believe there is a great deal in what Professor Hays has told us; that the principle which breeders of animals have found to be necessary, to breed, as they call it, from performance, should be used in our plant work.

The following paper was read by S. A. Beach:

CORRELATION BETWEEN DIFFERENT PARTS OF THE PLANT IN FORM, COLOR, SIZE AND OTHER CHARACTERISTICS.

S. A. Beach, Horticulturist, New York State Experiment Station, Geneva, N.Y.

Work in plant breeding, when considered from the standpoint of the worker, falls into two general classes. In one class the effort is to originate an improved strain or variety; in the other the object is to learn the philosophy of plant breeding, to discover the scientific principles involved and to illustrate the application of these principles. Immediate practical results are sought in one case; in the other a knowledge of the laws of plant breeding, by means of which continued progress may be made. The former, if successful, gives only something transient. The improved variety or strain which is produced will doubtless be superseded in time by something better, as the evolution of cultivated plants progresses. Such is the verdict of horticultural history. We look to the other kind of effort for the more permanent and eventually more rapid progress in breeding plants. It is the purpose of this essay to call attention to a question, the investigation of which may yield results of general importance and permanent value, namely, the extent to which correlation between different parts of the plant in form, color, size and other characteristics may be regarded as a significant factor in plant breeding. Nurserymen, seedsmen, fruit growers and gardeners know many instances of such correlation, and often take practical advantage of it when making selections for propagation, albeit perhaps unconsciously, and without formulating in words a definite expression of their judgment on this point. Scientific investigators have also occasionally recognized instances where correlation is of significance as a means of selection in breeding or propagating plants, but it appears that this subject has not been investigated as systematically and thoroughly as it should be.

In plant breeding the chances for originating an improved variety are in some degree proportionate to the number of seedlings produced. Other things being equal, the more numerous the seedlings subject to selection, the greater the probabilities of finding the improvement sought. But the more seedlings one undertakes to grow the greater the necessity of getting rid of the undesirable ones at as young a stage of growth as possible, and thus avoid the labor and expense of growing a great number of useless plants; therefore, a skillful breeder does not defer the process of selection till the seedlings appear, but exercises rigid choice, perhaps with the seed which is

to be planted, and surely with the parents, or stock plants, from which he expects to take the seed, cions, buds or cuttings for propagation. Correlation of parts or characteristics in plants may be of use not only in selecting seedlings, but also in choosing the parents, and even in choosing the seeds which are to be used in breeding. Notice a few illustrations.

Geschwind[1] finds correlation between the structure and the sugar content of the beet-root. He states that, as a rule, high sugar content is associated with a small amount of woody tissue, and recommends that beets be selected for breeding which have but small amount of woody tissue, as shown by cross-section of the top.

Henslow[2] states that McNab finds that in breeding rhododendrons the best dwarf varieties are obtained by using pollen taken from the smaller stamens.

Swingle[3] states that in Europe certain plant breeders who had long been engaged in breeding grain for the increase of the percentage of protein found recently that a high nitrogen content of the grain is correlated with blue stemmed plants, and since making this discovery have been enabled to make more progress in three years in increasing the nitrogen content of the grain by plant breeding than they had in many previous years' effort toward the same object.

Henslow[4] says that very dark crimson zonal geraniums are so nearly self sterile as to make seed raising difficult, the sterility being in proportion to the depth of color, which is correlated with proterandry. Paler varieties are more nearly homogamous and are very self-fertile.

Tinker[5] states that male vines of Vitis bicolor, Mx. at all ages, have leaves more lobed or divided than pistillate vines of the same species, and that this distinction is discernible in seedlings when they have put forth the sixth leaf. In his work in breeding grapes he finds it practicable to discard male bicolor seedlings when the sixth leaf is formed.

Debruyker[6] has shown that correlation in length exists between the culm and the head and the upper internode and the head of the rye plant. He did not find, however, that heredity had any apparent influence on these features. Many instances exist of correlation between different parts of a plant in size, but not enough observations have been made to permit of general statements as to the full significance of this character in any particular class of plants. De Vries[7] finds that there is a relation between the vigor of the plant in Oenothera Lamarckiana and the size, *i. e.*, length and thickness, of the fruit. The larger and more vigorous the plant, the longer and thicker the fruit; the shorter the fruit, the weaker and more slender the plant.

In the vineyard of E. C. Gillett, Penn Yan, N. Y., is a vine of the Con-

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[1]Geschwind, L., Rev. Gen. Chim. Appl., 3 (1900). No. 12. Cited in Exp. Sta. Rec. XIII: 526.

[2]Garden LX (1901); 228.

[3]Swingle, W. T., Discussion on Plant Breeding at the New Haven meeting of A. A. A. C. E. S., Nov., 1900.

[4]Henslow, G., Jour. Roy. Hort. Soc. XXIV: 85.

[5]Tinker, Dr. G. L., in personal communication to the writer, 1902.

[6]Debruyker, C., cited in Exp. Sta. Rec. XIII (1901): 241.

[7]De Vries, Hugo, Die Mutationstheorie. Erster Band (1901): 377.

cord grape, from one side of which has appeared a sport, bearing much larger fruit and much larger seed than is grown on the normal portion of the same vine. Seedlings which I have grown from seeds produced by the sport are larger and more vigorous in type than those produced from seed produced by the normal canes. None of these seedlings has yet fruited. The fruit of Hercules grape, a labrusca-vinifera hybrid, has very large fruit and correspondingly large leaves. The same is true of Columbian Imperial, Pierce and other varieties which might be named. Delaware has small fruit and correspondingly small leaves; so also have Golden Gem, Golden Drop, Rebecca and others. In breeding grapes I have found among some very excellent varieties others that were exceedingly dwarfed in leaf and habit of growth. When such dwarf vines have been allowed to mature and bear fruit they have produced either small fruit, or small clusters, or both. Many hundreds of grape seedlings of known parentage which have been produced during the progress of my work in breeding grapes show that size and color of foliage, vine and fruit tend to be transmitted to the offspring with considerable uniformity—so much so, that the entire lot of seedlings of any particular parentage, whether pure bred or cross bred, when viewed as a whole, is usually of a characteristic type and distinct from the seedlings of other, albeit nearly related parentage. Similar results have followed the work with gooseberries.

It should be remarked that in making observations on correlation of parts as to size it is important to give due consideration to the species or group features, if the individuals compared represent different groups. For example, some varieties of Vitis æstivalis Mx. may have larger foliage, but smaller fruit, than certain varieties of Vitis labrusca, yet within the limits of the species the larger types of leaf may be found associated with the larger types of fruit, and the smaller types of foliage with the smaller types of fruit.

With the peach it is easy to find many illustrations of a correspondence in size between the foliage and the fruit. Compare, for example, the type of foliage found on Elberta, Crawford and other large fruited varieties, with the smaller, narrower leaves found on smaller peaches, like Golden Prolific and Hill's Chili, and especially on the seedlings commonly called "natural fruit," which bear exceedingly small fruits.

Finally, on the question of the correspondence in size of different parts of the plant the evidence at hand, although not sufficient to support a general statement that it does exist, gives enough indications that it may be found to make the subject worthy of investigation.

The question of correspondence in color between different parts of the plant will now be taken up. In 1894, and again in 1897, a large number of varieties of apples in one of the orchards of the Geneva (N. Y.) Experiment Station were examined with reference to the color of the blossoms and blossom buds. Space permits but a summary of results. Two hundred and ten varieties were under observation. There appeared to be no constant relation between the color of the bloom and the color of the fruit, except that a large majority of the very pale or very nearly white blossoms were either on crab apples or Russian apples. One crab apple, however, was recorded as

having pure white blossoms, while its fruit is well described by the name of the variety, which is "Blood Red."

Raspberries, Rubus strigosus and R. Idæus and R. occidentalis, which bear so-called white or yellow fruit, have correspondingly paler foliage and paler canes than the black or red fruited varieties. So, also, the purple raspberries (R. occidentalis-strigosus or occidentalis-Idæus hybrids) have a distinct tinge on the foliage and canes corresponding to the purple color of the fruit. I have never known any exception to the above statements.

Some roses with white blossoms have noticeably paler foliage than that of dark red varieties. Similar correspondence in color of blossoms and foliage has been observed among pelargoniums, cannas, asters and other flowers. Apparent exceptions are seen among some of the cross-bred perennial phloxes.

Grapes with pale foliage, so far as I have observed, have so-called white fruit, or, at least, do not have dark colored fruit; but the converse is not always true, for some Concord seedlings which have the white fruit have foliage nearly or quite as dark as the parent. In observing this feature the fully matured leaves should be examined. Many instances are known of white fleshed peaches having correspondingly paler leaves and bark than have the yellow fleshed peaches. There is also often a noticeable difference between the foliage of varieties having pale yellow or lemon yellow flesh or skin and those having darker yellow flesh.

Emerson[1] writes me that there is a noticeable correlation between the color of flowers and the seeds of beans. Races, such as Jones, Davis, Navy, etc., which have white seeds, always have white flowers. Races that have black seeds, if memory serves me correctly, always have flowers that are strongly colored, *e. g.*, Challenger Black. Races that have spotted seeds or seeds tinted usually have flowers also tinted. One cannot always tell, however, by the tint of the flowers the exact degree of tinting or shading of the seeds. In the Blue Pod there is a correlation between the color of the flowers and foliage, as there is also with Scarlet Runner and White Dutch Runner. Seedlings of the last two are easily distinguished in one case by the reddish color of the stems, in the other by their light green color. Races with spotted pods, like Horticultural, usually have spotted seeds.

Fraser[2] states that when the young stems or sprouts of the potato are either green or white, it is an indication that the blossom will be white. If the stems are colored the blossoms will likewise be colored. The statement is based on observations of about 280 varieties.

Mr. C. W. Ward, Queens, N. Y., has called my attention to a correlation of color between the root and flower of the carnation, the white, yellow or red flower being associated with corresponding differences in the color of the root. Dorner writes me that he[3] has noticed that carnation plants bearing white flowers have white roots; those with yellow flowers have yellowish roots; those having the various shades of pink and red have pinkish roots, and those with crimson and purple flowers have dark roots of a dull purplish red. Plants having variegated red and white flowers show roots varying between pink and white. Crimson and purple varieties often show a purplish tinge at the nodes.

[1]Emerson, R. A. Horticulturist, University of Nebraska.

[2]Fraser, S. In personal communication to the writer.

[3]Dorner, H. B., Asst. Botanist, Purdue University.

The above observations are made only for varieties having solid colors, and exceptions may be found, but none have yet been noted.

In tulips the color of the flower may find a correspondence in the color of the bulb.

Lastly, let me quote from Mendel's observations on Pisum, as stated in his list of characters selected for his famous Experiments in Plant Hybridization.[1] Among other characteristic differences in the varieties chosen for the experiments he mentions those which relate:

"To the difference in the colour of the seed-coat. This is either white, with which character white flowers are constantly correlated; or it is grey, grey-brown, leather-brown, with or without violet spotting, in which case the colour of the standards is violet, that of the wings purple, and the stem in the axils of the leaves is of a reddish tint. The grey seed-coats become dark brown in boiling water.

"To the difference in the colour of the unripe pods. They are either light to dark green, or vividly yellow, in which colouring the stalks, leaf-veins and blossoms participate."

Finally, on the question of correspondence in color of different parts of the plant, as was the case concerning size, it may be said that the evidence at hand, although not sufficient to support a general statement that such correlation always exists, certainly gives strong enough indications that it may be found to make the subject worthy of investigation.

L. H. Bailey: Does the speaker find any correlation between the size of the leaves and the quality of the fruit? which after all is what we are after.

S. A. Beach: Not necessarily. I believe that we may find a correlation between the texture, and the texture of the fruit.

W. Bateson: In connection with this list of correlations which Mr. Beach has cited, there is a curious paradoxical case in the pea, Pisum sativum; the purple axil is correlated with a purple flower, while a white axil is correlated with a white flower. Curiously enough, in the sweet pea that is not necessarily the case. There are deep purple varieties of sweet pea which do not necessarily have a purple axil at all; but the contrary is not the case; there is no very white sweet pea which has a purple axil. I mention that as a paradoxical case which does not follow the same rule.

N. E. Hansen: In the fall of 1898 I called on Mr. Gideon at his home in Excelsior, Minneosta. For the benefit of Eastern people I will say that he has raised more apple seedlings than any one else in the West. He said that it he had a seedling in which the leaves were small he threw it away always, while any seedling with large leaves he kept; he had found that the tendency was toward larger fruit.

H. H. Groff: I would like to reconcile the reasoning of Professor Beach that the experimental worker is likely to overtake his more strenuous brother for the reason that the latter's efforts will soon be overtaken by his later and greater activity, I understand that that is the reasoning which he advocated, thereby sweeping away the experience of Mr. Burbank, Mr. Hays, and, I would like to add, myself.

S. A. Beach: I had no idea of undervaluing the work of the practical plant breeders; I believe in it thoroughly, and my only point was that I wished their assistance in trying to secure all the information possible, so that we can get at all data possible, classify it and put the whole business of plant breeding upon as nearly scientific a basis as possible. When we have done that, I think we can make more rapid progress than without it.

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[1]Mendel, Gregor. Reprint in Jour. Roy. Hort. Soc. XXVI. (1901);6.

A paper was here read by O. F. Cook, Botanist in charge of Tropical Agriculture, U. S. Department of Agriculture:

EVOLUTION UNDER DOMESTICATION

[This paper has not been submitted for publication, but the discussion thereon is given below.]

W. Bateson: The author of this paper raises a number of questions of very great theoretical importance. As I understand it, he proposes to distinguish variations according as they are debilitating, or, if I understand rightly, progressive. Such distinctions have, of course, in the past been attempted, but I feel that we should be better able to appreciate his position if he would indicate the tests by which he proposes to draw those distinctions. For my own part, I know no such tests, and I should like to ask him how he would propose to apply a test that would distinguish the debilitating variation from the progressive variation.

O. F. Cook: I am not trying to do what has been done in the past in that direction, but recognize the debilitation of a symptom of an evolutionary debility, if you will allow that expression (I don't know of one that will meet the case any better), and that those variations called mutations and also variations from many hybrids where the fertilization fails to grow, or is incomplete, are symptoms of organic or evolutionary debility. The test is not in the variation itself so much as in the history, as these things may be learned from our knowledge of the history of plants. On the other hand, there is, I think, probably a difference in the variations as noted; that is, there are (1) these abrupt variations without the gradations, and (2) the multitude of intergradations that we find in large species in nature which are freely breeding. We get these pronounced variations, which, I believe, are abnormal, and although there is no distinction to be drawn perhaps between the kinds of variation, there is a difference, as we might say, in the temperature of the body. We find a man lying on the street or somewhere; we find whether he is warm or not, and if he is cold we think he is dead. If he is warm we think he is alive; if he is extremely warm we recognize that he is ill. And so I think with this variation. It is a quality that is normal to a certain degree and may be abnormal in a greater degree.

W. Bateson: I want to know how we are able to recognize these differences. The speaker tells us that he will distinguish certain variations in a certain way, but I want to know how we can do it. What we want is a thermometer.

O. F. Cook: This, of course, is a question of degree. I think it is a matter that needs to be used with some sympathy, perhaps, to be appreciated. But I cited the coffee as a typical instance of a plant which, without selection, was producing relatively sterile variations. I may say also that I found excellent opportunities in Guatemala recently to see the tests of the varieties that have been advertised in coffee, and also to see a great many of these mutations that are exceedingly interesting, but much less fertile than the parent. The infertility occurs with offspring that produce a small number of berries, or that produce an abnormal number of berries, with but a small number of seeds, and so on through, and all these in the aggregate are less fertile than the parent form. Now, I am associating that fact with the historical fact of inbreeding, unintentional in this case, and associating it also with the general well-known fact that close breeding tends to sterility. That is a fact that I do not pretend to explain in the sense of a molecular or other cause. I am simply suggesting that association, and that association applies not only in the cases where it has been applied, but to a much larger extent than has been supposed. In the case of the wheat there is a distinction to be drawn. We

have not only the case of the wheat, but also the cases of plants, such as tropical fruits, like the banana, that have been propagated for thousands of years from cuttings. Those plants, with a strange uniformity, have declined in power of fertility. We ascribe that to selection, with no explanation of how selection brought this about, apparently; but if we ascribe it to inbreeding that seems to me a much more satisfactory explanation. In the case of wheat, of course, we have many other instances of that kind where plants have adopted a habit of inbreeding. I think that in many cases this is misinterpreted. These cases where inbreeding is common and frequent have been interpreted as universal cases. I believe there is a great distinction between a cross-breeding now and then in several generations, and a continuous and universal inbreeding. To what extent plants uniformly and continuously inbreed, I think is a question still open for discussion. However, supposing that that has become common and uniform, that fact, I believe, removes the wheat from the class of normal plants and those that are making good evolutionary progress, and places it alongside those that, like the banana, no longer produce seed, but vary, and their condition is no longer one of species, but of a mere fan-shaped divergence accompanied by very slight evolutionary progress. They are no longer bound together as species; they have abandoned that proposition. In other words, normal inbreeding, normal close-breeding, as in the case of wheat, would be a phenomenon to be associated with a sexual propagation, with parthenogenesis, and with other types of a sexual reproduction.

The Chair. I think the question of Professor Bateson is one in which we are all interested. It is this: Can Professor Cook give any definite test, specific test, that will cause a plant to be classed as a debilitated or a progressive variation?

O. F. Cook: Just in that form I do not consider the question a fair one. That is, the proposition is this: I have made the statement that there is such a relation between the inbreeding and the relative sterility, and the question is whether evolutionists, wheat breeders and otherwise, find that this relation holds good. If they do, why my whole contention is justified. It is not a question of test; it is a question of observation, and if the phenomena bears that interpretation, why, then, I think it may be a means of progress to make that observation.

D. T. MacDougal: I regret to say that I cannot find myself in accord with Mr. Cook in his statement concerning De Vries' mutation of species. I have been in rather close correspondence with Professor De Vries, and I am quite sure that Professor Cook is mistaken when he says that mutation species are sterile. On the other hand, there have been twelve species originated by mutation, and some of those are sterile and some are fertile, and this very mutation of species has produced more sterile and more fertile species. Now, as to general debility, whatever that may be taken to mean, I am quite sure that this debility does not show in the vegetative part of the plant. I have specimens growing in the houses of the New York Botanical Garden, and these forms may be exhibited to you to-morrow if you like, five of De Vries' mutation species, together with some of the original stock, and so far as the vigor—also a general expression which may mean a lot of things—so far as the vigor and general appearance of the mutation species are concerned, they are quite as strong in every way as the parent. I am not sure how far this affects Professor Cook's paper, and I find his entire paper rather significant and quite interesting. It is quite apropos that I should receive this morning a letter from Professor De Vries, dated the 20th of September, in which he speaks of his mutation experiments with Œnothera cruciata, a narrow-petaled form from the Adirondacks, which he has had under cultivation for some time. He writes to ask me if certain changes ever take place in this plant in America. I have just consulted with Dr. Britton, who, perhaps, has studied this plant as thoroughly and as long as any man in the country, and Dr. Britton tells me that he has never seen these changes, and so far as he knows they do not occur in this country. This, therefore, I think, justifies me in announcing at this time that Professor De Vries has succeeded also in obtaining a mutation species in Œnothera.

W. Fawcett: I did not understand Professor Cook's statements about coffee and the effects that breeding might have upon the increase of the size of the berry. Nothing whatever has been done in the West Indies in that direction, and I think probably that is the reason why the coffee has been so full of vigor and less liable to disease than in some other parts, like Ceylon. But the plan of getting young coffee crops is this: During a great part of the year the crop does not pay to pick. There are always some ripe ber-

ries on the trees; these drop, germinate, and take root, and then the planters choose the best and strongest of those "suckers," as they call them, for replanting. Now that is a natural selection, which, I think, has had great influence on the vigor of the trees, and, probably, if the planters made nurseries and germinated all the seeds from a tree, whether they were constitutionally strong or weak, they would probably not get as good results. But I think the paper of Professor Cook will certainly do a great deal to open the eyes of coffee planters in the West Indies to the question, and probably something will be done to test his theory in every way. I think the West Indies are much obliged to Professor Cook for taking up these tropical subjects.

W. M. Hays: I am exceedingly interested in this paper. We need studies in the theory along this line. I have had charge of some experiments for a dozen years in wheat, which, as I have already said, is close-fertilized, close-pollinated; and I want to emphasize one of the general features he brought out in his paper by an illustration which the wheat makes possible. We have in Minnesota two varieties of wheat that are almost universally grown, the common Red Fife and Velvet Chaff or Blue Stem wheat. On the University farm, where I am located, Fife wheat has averaged, we will say, 22 bushels for ten years, and under exactly similar conditions Blue Stem wheat has averaged 24 bushels. We take large numbers of these and plant the seeds, take choice seeds from each, selecting the choicest plants, and selecting from those the choicest seeds, and plant large blocks, we will say 5,000 for convenience, one seed in a place, four inches apart each way. We have devised a machine for planting them, so a man puts one seed in each of fourteen cups and throws the fourteen seeds over at one time. Another man throws the machine forward four inches, so that the wheat is planted under very uniform conditions. At harvest the choicest plants one way and another are gotten at, those that yield best and have good quality. Those are tested in the nursery for three years, by growing 100 plants from each of these mother plants. We will say we start with 100 mother plants, chosen as the best plants out of 5,000. These are run for three years, and the best 100 plants are chosen each year. At the end of three years, then, we have the measure of each 100 mother plants, not in terms of its own yield, but in terms of the yield of its progeny, of the average of its progeny. These plants are taken and the number is counted and the whole is threshed, and the total weight of the wheat is divided by the exact number of plants harvested. We will say, for example, that one mother plant yields a progeny that average 7 grams, another one 6 1-2 grams, another one 6 grams, and so on, variations running down, we will say, from 7 grams down to 4 grams. Then we pick out the best from that and take varieties. These varieties are grown then for three or four years alongside the plants of the two foundation varieties. The possibilities of getting a high yielding variety lie, as President Northrop quoted somebody in expressing the idea, in the projected efficiency of each of those 100 original mother plants we start with. We gradually throw away those with the least efficiency and finally take a comparatively few from the field that have the greatest projected efficiency, the greatest ability to project their peculiar heredity into varieties that will yield well in the field. We will assume for the purpose of the point I wish to make that we can raise the yield this way three bushels in each case; the Fife wheat we can raise from 22 to 25 bushels, and the Blue Stem from 24 to 27 bushels. Now, we can raise that by using large numbers and getting the plants that will make the largest yield, that have the highest projected efficiency, if you please, and then we can go a little further. If we use larger numbers we may add a little to that 25 and 27 bushels, and I may say that it has proven perfectly practicable to make varieties from single plants of wheat. Whether these will remain with their high ability for a long period of time, of course only time can tell, and it may be better perhaps to make the varieties from a number of foundation mother plants; but we have certainly run for ten years, and their ability to yield well has kept up. Assuming, then, that the yield is 25 bushels in one case and 27 in the other, we go back now to the matter of hybridizing these two varieties. The average between these two varieties would be 23 bushels. When we hybridize them we get in many cases plants that on the average are away down the scale, that perhaps would not have a yielding ability of over 10 or 15 bushels; in some cases we may get 20 bushels or even 23 bushels, possibly in some cases higher: but if we get a single plant with a higher yield, that is sufficient, because in the hybrids we take single plants again, and if we can get an occasional plant that has a projected efficiency that will give us a yield higher than 23 bushels, we can then get from that hybrid varieties that yield still more than from either of the simple stocks from which we started. Now, this experience

has led me to rather feel that this is a simple statement of the whole proposition that we are up against in our breeding. We want to use those means that create variation, and we want to use the best stocks we can get. We want to tip the land, as it were, and we even tip it at the lower end of the line far more than the other. We have hybrid varieties, many of them, in which there are no very good plants, plants with a very poor yield, but we have occasionally a hybrid that has some plants that are exceedingly strong in the combination of qualities we desire.

D. Morris: Here is one point in regard to what Professor Cook has laid before us this morning. He has cited coffee as an instance of a plant which has been for years under cultivation, and that there is little sign of variation amongst the coffee plants now cultivated in different parts of the world. As regards the New World, I should think that is owing to the fact that probably all the plants of the New World that are under cultivation now were originally obtained from one single plant which was brought to Jamaica, and from there coffee plants were distributed to various parts of the New World. Then there is another factor. When the coffee industry is being established, seed is taken from healthy trees, and once the estate is established the trees last for anything from thirty to forty or fifty years. So that the opportunity for variation from seed is comparatively small. There are some trees now growing in certain parts of Jamaica that are estimated to be anywhere from 60 to 80 or 100 years old; consequently, the coffee plant is different from an annual, because there the planting and the opportunities for variation are very considerable; whereas in the coffee plant, first of all started from one individual plant brought into the New World, and also the fact that the opportunities for variation are so small when once the trees are established, the situation is different. And I will mention that there is no plant so variable as coffee when once brought into contact with other forms. We have in cultivation in different parts of the West Indies the common Arabian coffee; as a variety of that I know a plant generally called Mocha coffee, with small berries; then a species was introduced from West Africa called Liberian coffee; then lately we have had a small berry coffee from Sierra Leone. Those being grown together in many gardens at the present moment, we cannot get any of those coffees perfectly pure from seed. The other day I saw a bed of coffee seedlings, and it was almost impossible to say what those seedlings were. I do not state these facts as an argument against what has been advanced by Professor Cook, but I think it is valuable to place on record the circumstances connected with the coffee plant as it is now being cultivated in the West Indies. I know that in estates in Jamaica there are variations between the coffee plants; there are some with rather large leaves and that yield good crops; there are others with small leaves that yield small crops, and so on through many variations. Then we have other trees that, instead of the horizontally spreading branches that the coffee tree usually has, have an erect habit, and those are often called by the planters male trees. In fact, I have been in Ceylon for many years, I have been in the West Indies for many years, and I have been very closely connected with coffee cultivation, and I should say that if you were critically to examine the coffee plants from different parts of the world I think you will find a good deal of variation amongst the individual plants at the present time. But, generally speaking, Professor Cook is quite right, that, looking largely at the coffee as grown in Central America and the West Indies, the coffee is fairly uniform in character. But still I don't know whether that is a good illustration to support his arguments. We have the fact that the coffee has probably been derived from a single plant; we have the other fact that establishing plantations of coffee can only take place at very long intervals, and coffee planting has been extended of late years only in Mexico and Central America. So, if we look for variations in the coffee trade we must look in those countries where is has been more lately introduced.

W. Fawcett: I would like to say there have been some considerable additions to the coffee area under cultivation in late years, the last ten years or so.

D. G. Fairchild: I should like to have Professor Cook elaborate briefly a sentence which reads as follows: "As organisms increase in complexity, they are less able to persist in a simple series, but require the interweaving of different lines of descent." Has this any bearing upon the fact that has been so clearly brought out by Mr. Burbank, that his best results in breeding have been with sports of plants which were represented by a large number of different so-called species? His plums, many of them, he told me, would

represent six botanical species; the fruits embody the blood, so to speak, of six distinct types.

W. H. Evans: Bearing out the remark of Dr. Morris, recently I got a report from the Hawaiian Islands, and one of their most prolific varieties, and what they consider their best coffee there, is what they call Horner's Guatemala, which is a variation from the Guatemala coffee. It is being grown by Mr. Lewison, and he considers it not only the most prolific, but it has the best flavor.

O. F. Cook: Guatemala is an extremely favorable country for coffee culture. Their coffee is magnificent, and that it should have greater vigor, etc., than other coffees from some other regions that I have seen I can readily imagine. My contention in regard to coffee was that, while coffee was an extremely uniform and stable plant as compared with any wild species, yet when it did vary the variations were pronounced, and it was this pronounced variation, accompanied by relative sterility, that I noted as the facts that I wanted to bring into relation. In relation to the question of sterility, I thought I had made it plain in my paper that this is a relative question. That is, Professor De Vries, and I think Dr. MacDougal, will have to admit that the average fertility of nearly all mutations is far below that of the parent, I think that is the case, and it is so represented by Professor De Vries himself. Then, furthermore, I have asked my evolutionary breeding friends to produce a mutation or a sport so-called which exceeded the parent in reproductive fertility. I am aware that the vegetative fertility or vigor is a matter that is often very slightly conneced with the reproductive strength. We have that in the case of the banana, for instance. That and other plants that have been in cultivation in the tropics for a much longer time than other plants have been under cultivation have tended very much to a lessening of the fertility from seeds. With reference to Mr. Fairchild's question, I would say that I have associated those two facts, that is, the wide distribution of plants with recent evolutionary progress; and the subsequent segregation of species among those plants is itself an indication that when those plants are crossed you restore at once this greater species evolution, so to speak. At any rate, whatever would take place in such plants would depend upon the divergence that had already been attained. If the divergence is too great, then you would get even in those plants relatively sterile hybrids. Of course, they may produce an extraordinary and much greater abundance of fruit.

The following paper was then read by Will W. Tracy, of Detroit, Michigan:

VARIANT TENDENCY AND INDIVIDUAL PREPOTENCY IN GARDEN VEGETABLES

W. W. Tracy.

SYNOPSIS.

The examination of a great number of plants will sometimes reveal what would not appear from a more careful study of a few.

1. Different plants of the same natural order tend to vary along parallel lines.
2. The natural orders are distinctly but differently influenced by conditions of soil and climate.
3. Cultural and climatic conditions are cumulative in their influence and affect the whole species.
4. The variant tendency in a race is common to different stocks and peculiar to each season.
5. Seed of the same stock and equally well grown, under precisely the same conditions, differ in adherence to type in different seasons.
6. Seed of individual plants of the same pedigree, grown under the same conditions and equally adherent to type, differ in prepotency or ability to reproduce themselves.

I do not claim to be a scientist, or that any investigations I may have made have been conducted in a strictly scientific manner, particularly as to their records, and my only excuse for occupying your time is that I have had exceptional opportunities to observe a vast number of plants of different garden races, both as to their variant tendencies and the influence of conditions of heredity and environment. For the past twenty years I have annually examined, for the purpose of detecting variation and the influence of heredity and environment, some 400 acres of tomatoes, 1,000 acres of cucumbers, 5,000 acres of garden beans, 200 acres of cabbage, and corresponding quantities of other garden vegetables and flowers. These crops have been grown in widely separated fields, under different conditions of soil and climate, and most of them from seed with whose pedigree I have been familiar, in some cases, back for ten or more generations. It seems to me that such extensive observation of an immense number of individuals, developed under varying conditions, might give hints of certain facts which would not be revealed in a

more critical study of the comparatively few specimens to which an intensive study is necessarily limited, and the purpose of this paper is the statement of some convictions in regard to racial variation which have resulted from such observations.

1. The different plants of the same natural order tend to vary along parallel lines. Fruits of the variety of tomatoes known as Early Conqueror, those of the pepper known as Squash or Tomato shaped, and of the Scarlet Fruited egg plant, could be selected, which would be as much alike in form as fruits from a single plant of any one of them; and I have seen a "potato ball" of the same form. I have found fruits of squash, muskmelon, watermelon and cucumber each having the peculiar forms and markings generally confined to one of the others. Thus, last year, I found a plant of watermelon whose fruit was distinctly warted, and in form would pass for a fairly typical one of Summer Crookneck Squash. I have seen muskmelons as flat and deeply scalloped as a fair sample of White Bush Scalloped Squash—squashes as well netted and distinctly ribbed as a Bay View Muskmelon. And it is taste and usefulness rather than limitation of variant tendency which determines the common shapes of each of these vegetables. I believe that hybridization is often credited with variation which is due to this common variant tendency.

2. The natural orders are distinctly, but differently, affected as to the character of their seed product, by conditions of soil and climate. For instance, if sweet corn, from the same ear, be planted where it will be subjected to different conditions of soil and climate, for but a single generation, the seed product will give plants differing materially in both stalk and ear. We think the same thing is true of wheat, oats and other gramineous plants. But we have never been able to detect the least difference in the character of seed grown from the same stock under different conditions in cucurbitaceous plants. We once planted a 10-acre field with Round Icing watermelon, five acres with seed which had been grown for four generations within 100 miles of the Gulf of Mexico, and the balance with seed, originally of the same stock, which had been grown for five generations in Michigan, and the most careful examination could detect no difference in the crop produced, either in earliness or other characteristics. Quite distinct varieties of melon are common at the North and South, and sometimes northern and southern strains of the same variety are quite distinct, but we think that this comes from the selection of the sorts best suited to the climate and to difference of ideals, and consequently in selection of seed stock rather than from influence of climate.

3. Cultural and climatic conditions are cumulative in their influence, and affect the whole species.

Thus, the Lima bean, originally a climbing plant, continued so for many years, during which time several distinct races were developed, but no dwarf form appeared; then, within three years, dwarf forms of all the different racial types appeared, and in several different places simultaneously. The sweet pea, cultivated for many years and closely watched by many enthusiasts, gave only climbing plants until 1892, when the "Cupids," or dwarf forms, appeared in at least three locations and different stocks and five individuals, and since then they have appeared in a great many different stocks and places, and

often where there could not have been pollen influence to induce the sport. In most vegetables, if any new form, no matter how distinct from those commonly cultivated, appears in one stock and place, there is almost a certainty that practically identical variations will appear elsewhere. For instance, the Navy Blue Sweet Pea was a very new and distinct shade, and appeared in the fields of two cultivators the same year, the only discernible difference in the two sports being that the seed of one had a greater tendency to skin-crack than that of the other. This tendency to sport into new forms developing in the species rather than in any particular stock is often the cause of much annoyance to seedsmen, two or more of them being accused of sending out a new form under different names, when each supposed that he had the only origination of that type.

4. The variant tendency in a race is common to different stocks and peculiar to each season.

For instance, in 1896, a distinct tendency to neckiness was noticed in Long Green cucumber; this increased in 1897, when I found several plants, all of the fruit of which more or less closely resembled that of the Summer Crooknecked Squash in shape. This tendency then gradually disappeared, giving place to one toward thicker fruit with white spines.

5. Seed of the same stock and equally well grown, by the same cultivator, in the same location, differ in the variant tendency, and the degree to which their product will be of the desired type in different seasons. The crop of seed of Green Globe Savoy Cabbage produced by a certain grower in 1893 gave much more evenly typical plants and heads than any subsequent crop produced by him of the same strain, though he took the greatest care in selecting stock and growing the plants, even setting them in the same field that gave the superior crop. I have known a practical seedsman, one not likely to waste money on a mere theory, to pay treble the market price for a certain strain of peas produced by him four years before, though he had an abundance of seed of the same strain grown by himself in succeeding years—none of these later crops giving such good results as seed of that particular season.

6. Seeds from individual plants of precisely the same pedigree grown the same season in the same field and equally true to the desired type vary in the degree to which their product will adhere to that type. I have gone into a field of Beauty tomato, of which every plant was from seed of an ideal plant selected the year before from a field similarly grown, and spent hours in picking out five ideal plants, and succeeded in getting those so nearly equally of the desired type that I could not distinguish one from the other; sowed and planted the seed separately and found that the seed of one of these gave fruit quite distinctly inferior to that of the general crop of seed; another gave fruit much superior, while that of the others was intermediate in quality. This is but one of scores of similar experiences which have convinced me that the most certain, if not the only, way to secure a high degree of uniformity and excellence in a race of vegetables is, first, to form a definite and distinct idea of what the race should be; then by selection *and testing,* find not only an ideal plant, but one of the greatest possible prepotency or ability to reproduce itself, and to multiply the descendants of this plant as rapidly as possible

until the entire stock is the lineal descendant of that individual plant—guarding against degeneracy by a never-ceasing search for other individual plants of equal or greater potential excellence, to be in turn increased.

O. F. Cook: I want to say that I speak of coffee as an illustration, not as an argument, and I tried to avoid, as you may say, the use of conflicting instances because of the variety of interpretations that could be put upon them. But I want to claim Mr. Tracy's as an instance of an application of my theories, and, furthermore, that they can be tested by such facts, and that I can accept Mr. Tracy's facts as normal and as actual, and I don't believe that the current theories can accept such facts without violation to the assumptions that are made on them.

The Chair: I am sure we all feel very grateful, indeed, to Mr. Tracy for his paper. It has a special value because of the fact of his connection with seed interests in this country, and his very great opportunities for observation. Probably there are few whose experience is so extended as that of Mr. Tracy, and I am quite sure that when his paper is published we will read it with very great interest, and possibly our scientific friends may, from the account which he gives, be able to reach some conclusions that they might not otherwise have formed.

D. G. Fairchild: Mr. Tracy cites an example of a tomato in which five plants were chosen, and the progeny from those five plants varied greatly. Were those plants self-fertilized, or is it possible that the male parent may have influenced the progeny differently?

W. W. Tracy: The plants were in a single field side by side, and they may have been cross-fertilized, but they were from the same blood line exactly. In this particular case of tomato I know of five generations from the same plants.

W. M. Hays: It has appeared to me that a plant like beet is entirely self-fertilized. There is evidence in a general way, and I think some positive evidence, that wheat does occasionally cross-pollinate, and it may be due to these occasional crosses that wheat is invigorated, and that some one plant among the crosses produced in nature finally dominates, increases more rapidly and becomes the major part of a new variety.

W. Bateson: I heard Mr. Tracy's paper with the greatest possible interest. I have been experimenting with the Cupid sweet pea, and I think the possibility attaches itself at once that it is a pure form, which may possibly explain the reappearance of that form subsequently and simultaneously in different localities. If, for example, the Cupid may once appear in a seed grower's field where it could get crossed on to another sweet pea, then such crossing does occasionally happen. In our country the crossing of sweet peas is occasionally accomplished by the leaf-cutting bee. If the pollen of a Cupid sweet pea were to get on the flowers of a tall growing sweet pea, then the seed might be scattered all over the country by the seed grower, and it would not appear the second year. Then the third year the seed might be distributed, and then the Cupid might appear. Of course, the simultaneous appearance of the Cupid in the third year of its sowing is a strong evidence that once a form has appeared its appearance in three years is explicable.

The following paper was read by D. Morris:

IMPROVEMENT OF THE SUGAR CANE BY SELECTION AND CROSS FERTILIZATION

Sir Daniel Morris, K. C. M. G., M. A., D. Sc., F. L. S., Imperial Commissioner of Agriculture for the West Indies.

The consideration of the improvement of the tropical sugar cane is in some respects out of the line of the majority of the subjects presented at this Conference. It may be convenient, therefore, to afford some information in respect to the conditions under which the cultivation of the cane is carried on in the West Indies and elsewhere.

The sugar cane is a tall-growing grass with a solid stem, containing a sweet juice. Its original home is unknown, but probably it was native of some parts of the East Indies and the islands of Polynesia. It is now believed to be found nowhere in a wild state. It was introduced from the East, by the way of the Mediterranean, to the Canary Islands, and thence to the New World. It was largely planted in Brazil, and about 1640 it was introduced to Barbados. The descendants of the original canes are probably still to be found in some parts of the West Indies.

In the latter part of the eighteenth century a ship was commissioned by King George III. and sent to the Pacific (South Sea) Islands to collect and convey to the West Indies sugar cane and other economic plants. These were afterwards established at St. Vincent and Jamaica. By this means what are known as the Otaheite and Bourbon canes were successfully introduced to cultivation. These, and various forms that have arisen from them, have since been adopted as the standard canes in most of the sugar-cane countries in the New World.

VARIETIES OF CANE.

The chief varieties now under cultivation may be broadly grouped as follows: (1) *Otaheite,* known also as Bourbon, Loucier, Lahaina, Bamboo, China; (2) *White Transparent,* known also as Caledonian Queen, Rappoe, Cheribon (Java), Crystallina (Cuba), Light Purple, Rose Bamboo; (3) *Red Ribbon,* known also as Mexican striped, Batavian striped, and striped Singapore. There are, besides dark purple canes, such as Queensland Creole, Black Java, Louisiana purple, Purple transparent.

The mode of origin of most of these is now impossible to trace. They have probably arisen from bud variations or from chance seedlings that attracted the attention of planters and preserved on account of their greater vigor and adaptability to environment.

MODE OF CULTIVATION.

Until about thirteen years ago the sugar cane was entirely propagated by cuttings or slips. There are buds, with sleeping roots, arranged alternately at each node. These render propagation by cuttings simple and effective. Generally the top joints only are selected, but sometimes the whole cane is laid in a trench and buried. The canes may be grown in isolated clumps or stools, about five to six feet apart each way, or continuously in furrows. The period of growth, depending on the climate, may be limited, as in Louisiana, to eight months; or extended, as in the West Indies, to twelve or even sixteen months.

IMPROVED VARIETIES.

The problem to be solved in behalf of the sugar cane planter, briefly stated, is to raise strains or breeds of canes that will give more sugar per acre than is at present yielded by standard varieties, such as the White Transparent and other sorts. This may be accomplished (1) by an increase in the weight of cane produced per acre, either by obtaining larger individual canes, or by a larger number of canes in each stool; (2) by an increase in the amount of sucrose in the juice with a reduction in the percentage of glucose and other impurities; (3) by freedom of the canes from the attacks of fungoid and insect pests.

The search for improved varieties has been carried on as follows: (a) by the introduction and experimental cultivation of selected canes from other countries; (b) by the experimental cultivation of canes (sports) arising from bud variation; (c) by the chemical selection of tops from individual canes or from stools exhibiting a high sucrose value; (d) by raising new varieties by cross-fertilization and selection.

QUALITIES OF IMPROVED CANES.

Before proceeding to describe what has been attempted under each of the heads above stated, it is desirable to mention that owing to the special circumstances existing in tropical countries a new variety of cane in order to be completely satisfactory is required to possess, in a high degree, a large number of qualities.

The following are the more important field and factory characters, a consideration of which goes to determine the ultimate industrial value of any variety of cane:

Field Characters: (1) Sprouting power of bud, and ability of cane to establish itself soon after planting; (2) behavior and adaptability under extreme conditions of dryness and moisture; (3) habit of cane, whether upright or recumbent; (4) power of resisting the attacks of insect or fungoid pests; (5) early maturity; (6) productive power estimated by the number of tons of cane yielded per acre; (7) weight and character of tops for fodder purposes; (8) readiness to produce successive crops from the same stools, that is "rattooning" power.

Factory Characters: (1) The milling qualities of the cane, whether tough or brittle, when presented for crushing; (2) fuel-producing properties depends on amount of fibre (sometimes the fibre is the only fuel available to boil the juice); (3) the relative percentage of expressible juice (determining the

"dryness" or juiciness of the canes); (4) the richness of the juice in sucrose; (5) the purity of the juice—that is, the absence of glucose, etc.

SELECTION OF EXISTING VARIETIES.

During the last forty years collections of selected sugar canes obtained from various countries have been maintained under experimental cultivation at Java, Queensland, Mauritius, and in the West Indies. This was the original form of research in the endeavor to obtain richer and more hardy canes. In Jamaica, from 1875 to 1886, there were about sixty varieties of named canes cultivated at the Botanical Gardens in that island. A list, with description, was published in the Report of the Director for the year 1884. Similar collections were also under experimental cultivation at Trinidad, British Guiana and Barbados. Francis at British Guiana and Harrison at Barbados were the first to start chemical investigations with the object of carefully determining the sugar contents of the cane and estimating the relative merits of those under cultivation in the West Indies. These investigations were started about 1883, and have been confirmed by Harrison and others until the present time. The results of the examination of existing varieties has not led to any very striking or definite results. In countries where the Otaheite and Bourbon canes are still free from disease, and the conditions are favorable for their growth. No other canes have, as yet, entirely taken their place. At Barbados, Antigua and St. Kitts, where the cultivation of the Bourbon cane has become impossible on account of its liability to fungous disease, other introduced canes are largely grown. There are also some seedling canes, but of these I shall speak later.

BUD VARIETIES.

A summary of information relating to canes raised as the result of bud variation (illustrated by colored plates) is published in the *West Indian Bulletin,* ii., pp. 216-223. Bud variation is not uncommonly met with in the sugar cane. Instances are recorded from Mauritius, Louisiana, Barbados, and Queensland. The differences between the sport and the mother plant are often as considerable as between the standard varieties of sugar cane. Bud variation may give rise to: (a) differently colored sports on the one cane; (b) differently colored canes in one stool springing from the same mother cane; (c) a single cane with some joints striped and others unstriped. It has been noticed that colored canes raised from sports tend to come true to color. Finally the canes that have hitherto given rise to sports are striped or ribbon canes.

In the matter of the distinctive character exhibited by sports it is recorded by Horne (Mauritius) that "most of the sports seem to be hardier than their parents and to yield more sugar;" Clark (Queensland) that "yellow sports have a tendency to grow sweeter than the colored canes of the kindred variety;" Stubbs (Louisiana) states, "the sugar contents of sports are fully equal to those of the ribbon and purple canes over which they have as yet no pronounced excellencies."

The systematic cultivation and testing of canes derived from bud varieties are being carried on by d'Albuquerque and Bovell, under the auspices of the Imperial Department of Agriculture at Barbados. The results will be published in the official reports issued by the Department.

CHEMICAL SELECTION OF PLANT CANES.

The selection of tops for planting from canes of high sucrose contents is sometimes described as chemical selection. This selection of cuttings may be made (a) from different parts of the same cane; (b) from a selected single cane out of many in the same stool; or (c) from a selected stool with a high average sucrose content.

There are some advocates of this method of improving the sugar cane. On the other hand Harrison is of the opinion that it is useless "to expend time, labor and money in attempts to raise improved canes by any system of selection of tops for planting."

It is advanced that the richest canes are simply those that are ripest and the best nourished; and that tops taken from such "richest canes" have a lower "germinating" power, and, possibly, are more liable, on account of their extra sweetness, to the attack of insect and fungoid pests.

SEEDLING SUGAR CANES.

Until within thirteen years ago it was generally understood that owing to the fact that the sugar cane for many ages had been propagated by cuttings or slips, it had lost the power of producing fertile seed. The flowering panicles (arrows) were often met with, but the seed was practically unknown. It is on record that canes, evidently grown from seed, were observed at Barbados in 1848 and 1850. At that time they were regarded merely as curiosities and no systematic attempts were made to grow them with the view of raising new varieties. The number of fertile seeds in each panicle is very small—possibly not more than 10 to 30 among several thousand spikelets—depending on the variety. Many canes produce neither flowers nor seed. What may be regarded as the effective discovery of seed in the sugar cane was made almost simultaneously by Soltwedel in Java and Bovell and Harrison at Barbados in 1888.* Fungoid disease at that time had attacked many of the standard sugar canes, and in both the East and West Indies energetic efforts were being made to raise new canes equal, if not superior to the existing canes, but less liable to disease. The discovery above referred to was greatly appreciated, and was immediately utilized. Seedling canes have in recent years been extensively raised. The difficulties to be overcome are considerable, but the experiments now generally carried on on scientific lines afford the hope that canes yielding a larger tonnage per acre and possessing high saccharine contents will eventually be obtained.

The accompanying illustration indicates the parts of the flower of the sugar cane, the character of the seed and the mode of germination; the references to the detail sketches are as follow:

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*Journal Linnean Society, xxviii., 197.

Sugar Cane: Parts of the flower, seed, and germination.
(For references see next page.)

EXPLANATION OF PLATE.

Fig. 1.—Portions of flowering panicle of sugar-cane, showing arrangement of spikelets.

Fig. 2.—A single spikelet enlarged (after Hooker); a = upper glume, b = pale, c = lower glume, d = anther, e = lodicules, f = ovary, g = stigma.

Fig. 3.—Ovary and stigma.

Fig. 4.—Caryopsis removed from glumes, with longitudinal and cross sections.

Fig. 5.—Caryopsis, showing first stage of germination.

Fig. 6.—Later stage of germination.

Figs. 7, 8 and 9.—Germination observed when the caryopsis is still enclosed in its glumes.

Fig. 10.—A seedling sugar-cane, natural size, three months old.

The experiments with seedling canes in the West Indies have hitherto depended, for the most part, on chance fertilization in the field, consequently only the seed-bearing parent is usually known. In further experiments it is proposed to secure cross-fertilization of selected canes as follows: (1) By planting in adjoining plots two varieties that arrow at the same time. while other canes are not arrowing in the same district; (2) by growing side by side, in rows, canes of different varieties that arrow about the same time, and afterwards bending over the arrow-stalks and bagging them before the flowers are open; (3) by bagging each arrow to be experimented upon some time before it expands and when the arrows in the bags are ripe to shake the contents of the bags of a staminate variety into the bags covering the arrows of the pistillate variety.

The majority of the best canes hitherto raised are of the first generation only. Seedling sugar canes, varying in number from two to twenty thousand, and even more, are now regularly raised in connection with the Experiment Stations in the West Indies. The first selection is made entirely from the field characters, but all subsequent selections are on the results of chemical examination. The number of selected canes that survive the tests up to the third or fourth year is probably not more than one in ten thousand.

It is impossible in this brief sketch to summarize the results of experiments carried on in all parts of the West Indies. Important features in these experiments are the hearty co-operation and the valuable assistance received from the planters. In all cases appreciable areas are established with new canes, and these are subjected to severe tests before they are recommended to be planted on a large scale.

The following is a description of one of the most promising seedling canes so far raised at the Barbados Experiment Station.[1] The results here given were obtained in the season 1890-1901 by d'Albuquerque and Bovell:

BARBADOS CANE NO. 208.

Germinates readily; from 10 to 15 canes to the clump; internodes from 3 to 5 inches long, somewhat cylindrical; color greenish yellow; habit upright; medium number of arrows; the dry leaves have a tendency to adhere; drought resisting.

Chief mean results per acre: Canes 26-24 tons, saccharose[2] 7,330 pounds (stands first in order of yield), juice very rich and pure, suitable for muscovado manufacture. Number of rotten canes below the average. Red soils

[1]Seedling and other canes at Barbados in 1901, pp. 23-25.

[2]Saccharose and sucrose are interchangeable terms for chemically pure sugar.

results equally favorable to the black soil results. Ratoons. This variety stood third in yield and gives saccharose 5,559 pounds, the juice being remarkably rich and pure.

This is the more promising selected variety under experimental cultivation this year, and deserves careful trial to the extent of a few acres on every estate.

Black Soils (plants and ratoons).

Highest yield, pounds saccharose per acre	11,298
Lowest yield, pounds saccharose per acre (mean of 2 plots)	5,492
Mean yield, pounds saccharose per acre (all plots)	7,766

Red Soils (plants and ratoons).

Highest yield, pounds saccharose per acre (mean of two plots)	8,336
Lowest yield, pounds saccharose per acre (mean of two plots)	4,369
Mean yield, pounds saccharose per acre (all plots)	6,623

Ratoons (red and black soils).

Highest yield, pounds saccharose per acre (mean of two plots)	6,728
Lowest yield, pounds saccharose per acre (mean of two plots)	4,369
Mean yield, pounds saccharose per acre (all plots)	5,559

Experimental samples of juice—results of 21 samples.

Saccharose lb. per gallon—Highest, 2.451; lowest, 2.028; mean, 2.254.

Quotient of purity—Highest, 93.98; lowest, 83.08; mean, 90.30.

Glucose ratio—Highest, 6.03; lowest, 1.35; mean, 3.11.

This seedling cane has yielded equally promising results in 1901-1902 at Barbados as well as at Antigua and St. Kitts under Watts, Shepherd and Lunt, and on a limited scale under Hart at Trinidad. The important experiments carried on by Harrison and Jenman for many years at British Guinea, and now entirely under the control of Harrison, are expected to yield results of great value to that colony.

It is impossible to omit a reference here to the experiments carried on with seedling canes at Java. The following is a summary of recent reports on the subject issued by Kobus at the East Java Sugar Experiment Station:

IMPROVED SEEDLING CANES AT JAVA.

In 1894 Dr. J. H. Walker, then Director of the East Java Sugar Experiment Station, found that the Cheribon cane bears infertile pollen while the ovary is normal. Consequently any fertile seeds formed by this cane are the result of cross fertilization by the pollen of another variety of cane, and the give rise to hybrid plants. About this time the new seedling canes raised were only those of well-known mother canes, *e. g.*, of the Cheribon. The Cheribon in Java, like the Bourbon is the West Indies, is a cane with rich and abundant juice and is therefore valuable as a sugar producer. Unfortunately, like the Bourbon, it is also liable to disease.

Soon after Wakker's discovery, Dr. Kobus, the present Director of the East Java Sugar Experiment Station, suggested the crossing of the Cheribon with certain of the East Indian canes, imported by the Java Government from British India, so as to raise seedlings from the former cane crossed by the latter, some of which would probably combine the good qualities of the Cheribon with the disease-resisting power of the East Indian canes. Experiments at this station were set on foot to cross the Cheribon with the Chunnee variety

from India, a very vigorous and disease-resisting cane. Dr. Kobus has published four reports on the results of the seedlings obtained from the Cheribon-Chunnee cross. In raising the seeds the parent plants were planted alternately in rows:

Cher.	Chun.	Cher.	Chun.
×	×	×	×
×	×	×	×
×	×	×	×
×	×	×	×
×	×	×	×
×	×	×	×
×	×	×	×

The reports are entitled *De zaadplanten der kruising van Cheribonriet met de Englesch-Indische variëteit* Chunnee, and were published as the Proceedings of the East Java Station Nos. 1, 12, 21, 33, of the Third Series.

The seedlings raised are carefully grown for four years at the station and compared with the Cheribon. The best are then distributed to the estates. The anticipations of Dr. Kobus have been realized, as canes combining both high sugar content and disease-resisting power have been obtained by this cross. The yield of sugar of some from the canes is said to vary from 6 to 8 tons per acre. In some cases the fecundating power of the pollen of the Chunnee is so strong that more than 95 per cent. of the hybrids resemble the male parent.*

D. G. Fairchild: Has that investigation of Dr. Stubbs taken this direction, as to whether there are occasional joints or buds which produce better yield, so that they can be taken as a unit, as Mr. Tracy mentions in his paper, and each one of those joints can be taken as a basis of a new variety?

D. Morris: I am afraid that Dr. Stubbs has not been able to go so far as that. What he has done so far, I think, he has followed the canes up to two or three years only. He has not been able to take them on a sufficiently large scale for that purpose.

S. A. Beach: Mr. President, I was somewhat interested in the account Mr. Morris has given of the methods of cross-pollination with the sugar cane, and it has reminded me of some results that were secured in my laboratory by Mr. Gould, which are presented in Paper No. 20, in which he found that it is possible to discover by microscopic examination of the pollen whether or not the pollen is fertile, whether or not the pollen is potent. That may be a point of some interest in selecting pollen-bearing canes, especially along the line that Mr. Fairchild mentioned yesterday, where they may choose to select them, and send them from one place to another. Possibly a similar condition may be found in the pollen of the cane. If so, it would be a point of practical value to determine by microscopic examination whether or not that variety would furnish pollen that is potent.

The Chair: Dr. Morris has certainly given us an exceedingly interesting paper. The subject is of very great importance to the world, though not of importance to the cultural operations of the Northern Hemisphere. I am quite sure that we most of us had the idea that sugar cane for some reason does not now produce seeds. But Mr. Morris has clearly shown us that, while it produces a very small quantity, it does produce seeds which are capable of germination and from which results may be expected.

D. G. Fairchild: I would like to ask Dr. Morris how large the plantations are at present in which these sugar canes of high sugar content are grown. I have met a number of sugar planters in different parts of the world who have heard about these sugar canes in the West Indies and Java, and their objection seemed to be that the plants

*Agricultural News, I., p. 146.

when grown on a large enough scale do not, as Dr. Morris has pointed out, furnish a sufficient quantity of cane, a large enough yield, and I would like to know how much, how many acres, for example, of this especial kind or breed of cane is now growing in the West Indies. I mean of 208 only.

D. Morris: Of this one cane that is now being distributed we have not more of this, I suppose, than about 20 acres in Barbados. Of seedling canes, generally, that is this one, 147, 95 and 109, I suppose in Barbados there may be 400 or 500 acres; in British Guiana I am not quite sure; I think they have something like about 1,400 acres; that is of the seedlings generally. Naturally the planters, and we are very glad that they are, are cautious in starting these seedling canes. If a man were to plant 100 acres in new canes and the return was a very small one, it would be a very serious item to him. We do not wish to increase the difficulty of the planter, we wish to help him; and we always preach great caution in adopting new canes for general cultivation.

The following paper was read by D. T. MacDougal:

SOME CYTOLOGICAL ASPECTS OF HYBRIDS

W. A. Cannon, Columbia University, New York.

Until recently research upon hybrids has been almost entirely confined to the more practical problems, such as are connected with the formation of new or better sorts of plants, and students of pure science have given little attention to the subject. At the present moment, however, much interest is manifested by scientists in this direction, and this awakened interest is largely due to the republication by Correns and De Vries of Gregor Mendel's experiments and results. The more recent studies have a two-fold nature. They are either experimental, as the well known work of De Vries, Correns, Webber and Tschernak, or they are cytological; and each phase of the work has been undertaken entirely independent from the other.

What are the relations of the cytological to the experimental researches of hybrids? This question can hardly be answered at this time, but the possible connection between the two may be pointed out, however.

Although it is not the purpose of this paper to set forth the conclusions of the experimenters, for illustration and for reference, it will be convenient to briefly present those of Mendel.

Mendel studied the behavior of hybrid plants, not only in the first generation—that of the immediate cross—but in the following ones as well, and learned (1) that in the first generation the hybrid shows the characters of one parent only, but that (2) the characters which were latent in this generation appeared in the later ones, and in such a manner that the plants having the "dormant" and the "recessive" characters bear a certain and definite ratio to one another.

In this connection it need only be said that while the conclusions of Mendel have been verified by the later researches, they have been found to be limited in their application to a special type of hybrid, namely, the Pisum type, while the greater number of hybrids belong to other types.

In the Pisum type the plants behave as if the bundles of inheritance which were derived from the parents of the original cross were kept separate, and were delivered as such to the succeeding generation, and, as if in fertilization, these were united in all possible proportions. This has been expressed by the following formulæ: A+A, A+a, a+A, a+a. The result is that a certain per cent of the hybrids revert to the characters of the parents of the original cross. Or, in other words, according to the laws of Mendel, we might expect that the chromatin derived from the primitive parents main-

tained its individuality, and was disposed in such a manner at the time of the maturation mitoses that the resulting sex cells were not *hybrid,* but *pure.*

A portion of this hypothesis has already been demonstrated to be true, namely, in hybrid *ascaris,* the chromosomes derived from the variety *bivalens* keep, separate from the one which is evidently from the *univalens* parent. And it will appear presently that an analogous condition may obtain in other hybrids.

That a hybrid sex cell has chromatin of pure descent—that is, has chromatin from one parent only, as the second law of Mendel apparently demands—is yet to be proved, and may be doubted on *a priori* grounds, since the wall separating the daughter nuclei is laid down at right angles and not parallel to the spindle of the dividing nucleus. However, if it can be shown that the *Pisum* type of hybrid, for example, is associated with the general behavior of the chromatin, as just suggested, a very important and forward step in explanation of the nature of this and other types of hybrids shall have been made. On the other hand, it will be quite as important to demonstrate that variation in hybrids may take place, even if the sex cells are formed in a manner identical with those in pure races.

Now that I have pointed out the possible relation between the experimental and the cytological study of hybrids, I hasten to say that, in reality, up to the present moment there has been no such connection, since the cytological work was mainly completed before the experimental was published; and, further, since the forms which have been turned to account cytologically have not been studied experimentally. With this I shall proceed to speak briefly of the cytological work.

This has comprised studies by Guyer on the spermatogenesis of hybrid pigeons and cannas, by Juel on the spermatogenesis of hybrid *Syringa,* and by the writer on spermatogenesis of hybrid cotton. In addition, the study on *Ascaris,* already sufficiently alluded to, and also the structural study of several hybrid plants by Macfarlane may be included here.

Concerning all of the plants upon which cytological studies have been made it may be said that they are first generation hybrids; that is, they are the offspring of the original cross.

Guyer reports upon the spermatogenesis of hybrid pigeons, and also cannas, but since the results are analogous and the conclusions identical I need only outline the results of his study of the former.

I understand that Guyer found in the pigeon normal and abnormal spermatozoa and normal and abnormal maturation mitoses. In the normal male nuclei the first maturation division showed ring-formed chromosomes, of uniform size, and of half the somatic number. He found, on the other hand, in the abnormal maturation mitoses that the ring-formed chromosomes might be of two sorts, large and small, or they might all be small; and if the latter, they were the same number as those in the stomatic nuclei. In such abnormal nuclei there was a tendency of the chromosomes to form into two groups, and this localization was taken to indicate the maintenance by the chromosomes of their individuality. The general conclusion of the author was, that the variation noted in the maturation mitoses was likely associated with the variation of the hybrids themselves.

Guyer had, I believe, like results from his study of hybrid cannas, except in the canna he found all gradations between direct and indirect divisions.

The writer has studied the maturation divisions in the male nucleus of hybrid cotton, which was supplied him by Dr. Webber. The results of this study may be summarized as follows: Two sorts of maturation divisions were distinguished, one clearly normal, the other abnormal. Concerning the former it need only be said that the ring-formed chromosomes of the first division were of uniform size and of the reduced number. The spindle gave no indication of being double, and was entirely analogous with that in the pure race. The abnormal divisions were all direct, but varied in a manner, of which it is unnecessary here to speak further. Transitions between direct and indirect divisions were not observed.

From my study I concluded that the normal divisions lead to fertility, the abnormal to sterility.

The remaining purely cytological study is that by Juel on the spermatogeneses of *Syringa rothomagensis*. Juel found abnormal mitoses only. These were all stages between direct nuclear divisions and indirect, save only that the chromosomes did not split, in the metaphase.

What is the significance of Juel's results? It is of interest here to note that the plant may be wholly sterile, since Focke observes that he has seen no fruit on it—that is, the abnormalities in spermatogenesis seen by Juel in the hybrid Syringa lead to sterility.

Thus Juel's results cause one to ask whether some, if not all, of the abnormal sex nuclei observed by Guyer in pigeon and canna were not functionless, and whether the normal divisions alone do not lead to fertility? Such is apparently the case in the hybrid cotton. If only sex nuclei that undergo typical maturation mitoses form functional reproduction cells, it is clear that the variation, in some hybrids at any rate, must be occasioned by other causes than those of the irregularities in the splitting, distribution and union of the chromatin of the sex cells.

Here it may be suggested that an experimental study of such a hybrid as shows a tendency toward maintaining the individuality of its chromosomes might be carried on with good results. Such a study should show the relative sterility of the sex cells, and thus it would supplement the results derived solely from the cytological investigations. This has not yet been done, however.

While the cytological data just given do not harmonize completely, further study may show that they are really not antagonistic, but that the different hybrids studied should either be placed in different hybrid classes, or that the "splitting" may not occur in the first, but later generations.

Thus the leading results which have been obtained by the cytological study of hybrids may be summarized as follows:

(1) The establishment of one cause of sterility among hybrids.

(2) The variation of the hybrid may or may not be associated with abnormalities in spermatogeneses; and

(3) That in certain cases the chromosomes derived from the original parent tend to preserve their individuality.

Before closing this brief account of the cytological studies in hybrids I

wish to speak of the work by Macfarlane on the structure of certain hybrids, a work published about ten years ago. After giving in detail the minute structure of several hybrids, this author arrives at this general conclusion concerning the nature of a hybrid, namely, that it is a blending of the characters of the parents, each parent contributing equally to the offspring. The hybrid is thus intermediate in structure between the parents.

Although it may be at this time premature to suggest a relationship between the results of studies in the structure and those from experiment, nevertheless it is interesting to note that practically all of the hybrids reported by Macfarlane are of the intermediate type of Correns, and perhaps, therefore, such results as Macfarlane obtained might thus have been expected. From this the possibility is suggested that hybrids which are not of the intermediate type may have a minute structure which is likewise not intermediate. No work on this phase of the subject has yet been done, however.

Finally, as has already been stated in the foregoing summary of the recent cytological work upon hybrids, this line of research and the experimental have gone on independently of each other, but it may be reiterated here that in order to better understand the causes of the variations in hybrids and that of the differences in capacity for variability it is highly desirable that the cytological and experimental work go hand in hand, that cytological work be done on forms that give marked experimental results.

A summary of the following paper was read by L. C. Corbett:

IMPROVEMENT OF ROSES BY BUD SELECTION, OR BLIND VS. FLOWERING WOOD FOR ROSE CUTTINGS

L. C. Corbett, Horticulturist, Department of Agriculture, Washington, D. C.

The results which are recorded in the article prepared under the above title are based on a series of tests with rose cuttings made from "blind" and "flowering" wood, and cover a period of five years.

The work was undertaken to settle a point in dispute among commercial growers of roses as to the relative value of plants grown from blind and flowering wood for flower productions.

VIEW POINT, OR QUESTIONS TO BE SETTLED.

1. Do cuttings tend to perpetuate the individual peculiarities of the parent branch from which they are taken?

2. Can accumulative results be obtained from a continuous use of cuttings from wood with like habits? i. e., can the flowering habit of plants be increased by the continuous use of flowering wood selected through successive years from plants which have themselves been produced from flowering wood?

For many years discussion has been rife among commercial growers of roses in regard to the flower or bloom-producing power of plants grown from what is known as "blind wood," and those grown from "flowering wood." These terms are familiar to all accustomed to the propagation and cultivation of the rose under glass; but in order that none may hold a misconception of the point in question, I present, (page 95), a figure representing the two types of wood spoken of. The branch at (a) in the engraving represents a characteristic shoot of the so-called "blind wood." This shoot, it will be noticed, is of slender growth, somewhat willowy in character, and is terminated by a leaf instead of a flower bud. Branch (b) is a flowering wood shoot, as is indicated by the terminal flower bud. As a rule, "flowering shoots" are larger and more vigorous, but are also softer and contain a larger percentage of pith in proportion to the woody tissue than "blind wood" shoots. In general, however, the "flowering wood" shoots are longer than the "blind wood" branches, and if the flowers are cut with short stems, then there is a considerable length of wood suitable for purposes of propagation at the base of each flowering shoot. This wood is harder and more mature than the wood close to the base of the bud, and for that reason is better suited for making cuttings. With plants which are blooming profusely it not uncommonly happens that

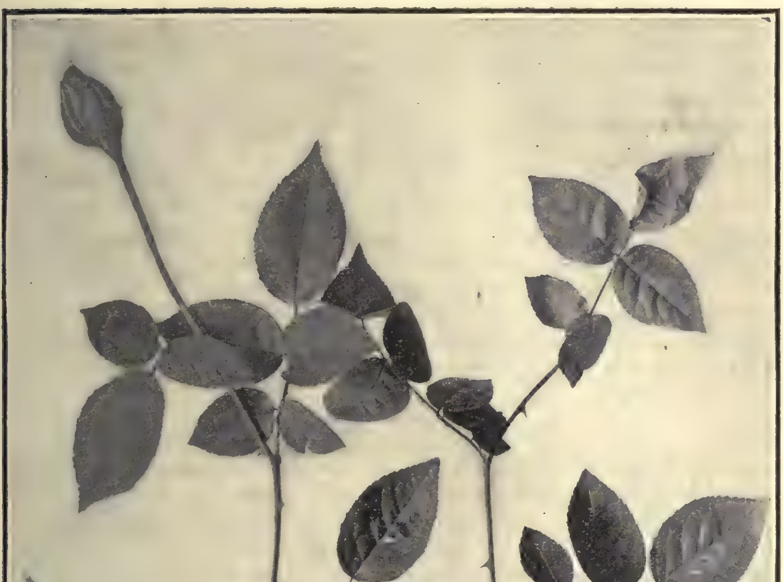
the whole length of the flowering shoot is needed to satisfy the market demand for long-stemmed roses, and the supply of suitable wood from which to propagate the next season's stock of plants is greatly lessened, or it may be entirely cut off. But there is always a greater or less supply of "blind wood." Consequently, why not use it for purposes of propagation? If the plants grown from blind wood do not perpetuate the tendency of the parent shoots (which are non-flowering), then there can be no objection to its use for the purpose of propagating the next season's stock. But if it does perpetuate the tendency of the parent stem, then there is danger.

In order that more definite statements may be made on this point, the writer has for five years carried on a test with rose plants from the two types of wood above mentioned. Before stating the results of this experiment, however, I wish to call to mind a few of the experiments which have been made which throw light upon the point in question.

Do cuttings tend to perpetuate the peculiarities of the parent branch from which they are taken? To answer this, I need hardly do more than call attention to the fact that propagation by cuttings is employed almost exclusively for the perpetuation of cultural varieties of all fruit and ornamental plants which are capable of being grown from cuttings. Many annual plants, however, come true from seed and varietal differences, while not so constant as in plants grown from cuttings are, nevertheless, sufficiently close for all commercial purposes. If we were to go a step farther, we might be justified in considering the various processes of budding and grafting as identical in their results with that of propagation by cuttings.

Budding and grafting are in reality processes of division, the same as is the growing of plants from cuttings. In all three of these modes of reproduction the results are so constant that we never stop to question the fact; yet we constantly commit the blunder of ignoring qualities quite as important as the varietal peculiarity itself. In fruit growing, nurserymen propagate from a Baldwin tree, whether it has ever borne fruit or not, simply because they know it to be a Baldwin. Yet in the face of this we are being taught by our advance agents that each tree has an individuality, and, in fact, that each branch and bud is in its peculiar way different from every other branch or bud, even upon the same tree. If we accept these statements as true, and we have no good reason to doubt them, then the peculiar tendencies of the plant, or of a branch of a plant, may be expected to play a more or less important part in determining the behavior of the plant or plants propagated from it. Orchardists have observed these differences, and are slowly coming to exercise greater care in the selection of cions. This precaution not only influences fruit production, but it has been clearly pointed out by Smith, Fairchild and others the health of plants from which buds and cions are taken measures to a very marked degree the health and longevity of the resulting tree.

If we find these differences among plants grown from buds and cions, quite as marked peculiarities may be anticipated in plants grown from cuttings. Upon this point recorded observations are exceedingly meagre, but some light can be gathered from the work published by myself in the Ninth Annual Report of the West Virginia Agricultural Experiment Station. From



A—Blind Wood. B—Flowering Wood.

these studies it is evident that varieties are perpetuated true to type by cuttings through many generations. A single exception in the case of the tomato entering to break the constancy of the results. Not only are general varietal differences retained, but acquired characters also, temporarily, at least, as is shown in the cuttings of grape, poplars, currants, etc., grown in Northern and Southern latitudes. For a specific example of this, nothing could be more conclusive than the results shown by New York grown potatoes, given one season's outing in Maine, for, when brought back to New York the next year, they retained their Maine tendency towards increased vigor and yield.*

Trees propagated from fruitful trees are themselves more fruitful. Potatoes grown a year in the North become more prolific. Plants grown from cuttings, taken from Northern and Southern grown parents of the same species, retain the characteristics of their parents. What, then, should be expected from plants grown from blind and flowering wood? To anticipate the results of the test I will state that they accord with and justify the logical conclusion which would follow from the particular instances above set forth. In this test rose plants propagated from "flowering wood" gave on the average 29 4/9 blooms per plant for the season, while the "blind wood" plants produced 11½ flowers per plant.

DETAILS OF THE EXPERIMENT.

In the Spring of 1897, when the time for making rose cuttings had arrived, cuttings were made from both flowering and blind wood of each of the varieties of rose then in the house. The cuttings were all made on the same day, placed in the same cutting frame in contiguous rows. In all respects the conditions for the several cuttings were as nearly the same as it is possible to obtain in a greenhouse.

On February 16 the cutting plants were examined and potted, with the following results:

Variety.	Total No.	No. Rooted.	No. Calloused.	No. Dead.
Bride, flower	15	9	6	.
Bride, blind	20	9	9	2
Bridesmaid, flower	9	5	4	.
Bridesmaid, blind	23	15	8	.
Perle des Jardins, flower	15	2	11	2
Perle des Jardins, blind	17	7	7	3
Mme. Hoste, flower	13	5	8	.
Mme. Hoste, blind	9	1	8	.
Meteor, flower	18	11	3	4
Meteor, blind	5	4	1	.

From this it would appear that there is little difference in the tendency to form roots between the cuttings made from flowering and from blind wood.

From this time until the plants were set in their permanent places upon the greenhouse benches (August 19, 1897), they were given like treatment. They were grown in pots in a sunny greenhouse, and all received the same number of shifts, and like attention in regard to soil, water and food supply.

*See Ninth Annual Report of West Virginia Agricultural Experiment Station.

As the plants were planted upon the benches August 19, the following note was made: "At this time there is little difference in the size and general vigor of the plants from the blind and flowering wood cuttings."

As soon as the plants became established in the soil on the bench growth on all was vigorous, and while there was a marked difference in the growth and general behavior of different varieties, there was no notable difference in vigor between the blind wood and flowering wood of the same variety.

The bloom record of the several lots is quite different, however, as is shown in the accompanying table, which gives the total number of flowers produced between December 1, 1897, and July 1, 1898:

Variety.	No. Plants.	No. Blossoms.	Average.
Bride, flower	13	242	18 8/13
Bride, blind	18	32	1 7/9
Bridesmaid, flower	8	192	24
Bridesmaid, blind	3	56	18 2/3
Perle des Jardins, flower	8	141	17⅝
Perle des Jardins, blind	14	224	16
Mme. Hoste, flower	7	204	29 1/7
Mme. Hoste, blind	9	176	19 5/9
Total No. blind	44	488	11 1/11
Total No. flower	35	779	22 9/35

As is seen from the table, the product of the flowering wood plants in this one year's test was more than double that of the blind wood plants, amounting in this particular instance to 156 per cent. gain in favor of the flowering wood plants.

During the forcing season of 1898 and 1899, extending over the same period as in 1897 and 1898, i. e., from December 1 to May 31, inclusive, the two varieties retained in the experiment produced bloom as follows:

1898-99.	No. Plants.	No. Blossoms.	Average.
Bride, blind	13	113	8 9/13
Bride, flower	15	253	16 13/15
Bridesmaid, blind	8	119	14⅞
Bridesmaid, flower	12	210	17½

The results here recorded show the superiority of the flowering wood over the blind wood plants. But strange as it may appear, the difference between the flower-producing power of the plants grown from flowering wood and from blind wood is less than in the first generation, and this, despite the fact that the plants used in this test were grown from cuttings selected from plants used in the previous years' tests.

HISTORY OF THE SECOND YEAR PLANTS.

The flowering wood plants used in the tests of 1898 and 1899 were grown from cuttings selected from the base of flowering shoots produced by the plants previously grown from flowering wood. This course was followed in order to test the effect of constant selection in one direction, the idea being to secure plants with the maximum blooming capacity. The experiment was

to test a theory which may be stated as follows: If flowering wood from commercial plants is capable of producing plants able to throw more than double the number of blooms produced from similar plants grown from blind wood, is it not possible by selecting flowering wood from plants grown from flowering wood to increase the flower-producing tendency in a distinct strain of forcing plants? Beginning with blind and flowering wood from good commercial plants of Bride and Bridesmaid in the Spring of 1897, plants were grown and flowered in the Winter of 1897 and 1898; from these, as above stated, cuttings were taken and flowered during the Winter of 1898-1899; and from these, in turn, came the flowering plants for 1899 and 1900, and so on for five years; the flowering plants being from flowering plants of the previous season in each and every case. The blind wood plants were treated in exactly the same way, blind wood cuttings from blind wood plants produced the blind wood plants for the succeeding season, and so on for the five years over which the experiment has now extended. The following records for each of the years 1899-1900, 1900-1901, and 1901-1902 serve to show how constantly the flowering wood plants retain their supremacy over those grown from blind wood:

Record from December 1 to May 31, Inclusive.

1899-1900.	No. Plants.	No. Blossoms.	Average.
Bride, blind	15	190	12 2/3
Bride, flower	18	192	10 2/3
Bridesmaid, blind	18	137	7 11/18
Bridesmaid, flower	18	220	12 2/9

Record from December 1 to May 31, Inclusive.

1900-1901.	No. Plants.	No. Blossoms.	Average.
Bride, blind	17	59	3 8/17
Bride, flower	4	110	27½
Bridesmaid, blind	16	67	4 3/16
Bridesmaid, flower	14	80	5 5/7

Record from December 1 to May 31, Inclusive.

1901-1902.	No. Plants.	No. Blossoms.	Average.
Bride, blind	14	80	5 5/7
Bride, flower	16	124	7¾
Bridesmaid, blind	16	98	6⅛
Bridesmaid, flower	12	136	11 1/3

RECORD OF PLANTS SECOND YEAR ON BENCH.

Record from December 1 to May 31, Inclusive.

1900-1901.	No. Plants.	No. Blossoms.	Average.
Bride, blind	14	193	13 1/14
Bride, flower	8	223	27⅞
Bridesmaid, blind	10	138	13 4/5
Bridesmaid, flower	12	183	15¾

*Cutting plants set on bench in July.

But, strange as it may seem, there is no apparent cumulative effect from the selection of cuttings from flowering wood plants. Neither is there any marked degradation from the continuous use of blind wood. True, there is a constant falling off in the average number of flowers produced per plant from the first season to the close of the experiment, but the result is not more marked in the case of the blind wood plants than with the flowering wood plants.

The following table, which summarizes the results for the five years, is of interest in that it shows but a single departure from the rule that flowering wood produces plants which are more floriferous than those grown from blind wood:

AVERAGE NUMBER FLOWERS PER PLANT FOR SEASON—DEC. 1 TO MAY 31, INC.

Year.	Bride (Blind)	Bride (Flower)	Brides-maid (B)	Brides-maid (F)
1897-98	1.88	19.84	19	28.62
1898-99	8.66	16.86	14.87	17.5
1899-00	12.66	10.66	7.62	12.22
1900-01	12.21	27.87	13.8	15.25
1901-02	5.71	7.75	6.12	11.33
Average for 5 years	8.26	16.59	12.29	16.98

This is seen in the case of the blind wood plants of Bride grown during the forcing season of 1899 and 1900, in which year this particular lot of plants produced an average of two blooms per plant more than did the flowering plants of the same variety. The table is of interest also in showing the ratio of the flowering wood plants to the blind wood plants based on the average number of flowers produced per plant during the season. In the case of Bride the blind plants averaged 8.26 blooms per plant, while the flowering plants produced 16.59 blooms per plant, or a little more than twice as many. In the case of Bridesmaid, the difference is very decided, but not so great as with Bride. Bridesmaid blind produces an average of 12.29 blooms per plant, while the "flowering wood" plants of the same variety produced 16.98, or one and one-third times as many as the blind wood plants.

COMPARISON OF FIRST AND SECOND YEAR PLANTS.

In the course of these observations a cultural problem of some moment presented itself, and as it could be brought under observation without deranging the observations on the production of bloom from blind and flowering wood plants, the experiments were planned to admit of retaining a number of plants upon the benches a second season in order to compare their flower-producing power with plants grown from cuttings and placed on the benches in July for the succeeding Winter's flower supply. Accordingly 14 blind and 8 flowering wood Bride, with 10 blind and 12 flowering wood Bridesmaid plants were retained on the middle bench of the house used continuously in this test. These plants were severely pruned in August, after having been kept quite dry and inactive during July. After pruning at least one-half of the soil of the bench was replaced by fresh compost. The earth was removed from on top the roots and between the plants and replaced with fresh earth. After this treatment they were slowly started into growth and the record of

flower production began in October. The following table will serve to show the bloom record of these plants:

Month.	Bride. Blind.	Bride. Flower.	Bridesmaid. Blind.	Bridesmaid. Flower.
October	57	49	51	49
November	25	29	15	20
December	39	36	33	33
January	14	18	9	19
February	28	26	18	29
March	46	47	26	41
April	39	47	34	33
May	27	49	18	28
Totals	275	301	204	252
Average per plant	19.6	37.6	20.4	21

In the case of plants grown from cuttings struck March 11, and planted on the benches in July, the record was as follows:

Month.	Bride. Blind.	Bride. Flower.	Bridesmaid. Blind.	Bridesmaid. Flower.
October	4	10	7	5
November	6	5	1	10
December	.	2	5	2
January	4	8	3	7
February	7	12	7	7
March	8	11	12	16
April	19	40	22	29
May	21	37	18	19
Totals	69	125	75	95
Average No. per plant	4.05	31.25	4.68	6.7

From the comparison of the average number of flowers produced per plant from October 1 to May 31, inclusive, in each of the two sets, it is evident that there is little difference between the two, but upon comparing the monthly flower product of the two it at once becomes apparent that the two-year-old plants produced their crop in the Fall and early Winter, while the cutting plants produced the heaviest bloom later in the season. One must, therefore, be guided by the demands of market. If a heavy crop before the holidays is the end to be achieved, then year-old plants are desirable, but if the rose market is more profitable from the first of February to the first of June, the cutting plants will give best return. While these results are interesting in showing the value of strong year-old plants, we do not consider that the record of the one season, during which this comparison was made, sufficient to be taken as a basis for extensive commercial undertakings. With the tests of blind and flowering wood plants, however, the case is different, and the results may be considered conclusive.

DEDUCTIONS.

It is clear from the results of these tests that the tendencies manifested in a branch are perpetuated from generation to generation in plants propagated by sexual processes.

It is equally demonstrated that cumulative results are not to be expected by selecting parts showing like tendencies through successive generations. The flowering habit of plants which themselves had been produced from flowering wood was not increased, even in the fifth generation, over what it was in the first. On the other hand, plants repeatedly propagated from blind wood through five successive generations were not markedly less floriferous in the fifth than in the first generation.

In both plants propagated from blind and from flowering wood, there was a slight tendency towards lessened flower production. This may be accounted for in that the stock from which the plants were propagated each season had been grown and forced under artificial conditions, and no attention was given to selecting cuttings from the strongest plants. The commercial side of this experiment is, of course, the most important one from the standpoint of the practical grower. It is clearly more economical for the florist to produce his plants each season from blind wood, and since there is no cumulative effect from such a procedure, the plants so produced are not necessarily less floriferous than the parent stock. But where bloom rather than stock plants is the end sought the tests above recorded are emphatic in declaring the superiority in this respect of plants grown from flowering wood. A rose grower can well afford to send short-stemmed roses to market during the months of January and February, if by so doing he can secure sufficient flowering wood for propagating purposes to insure a stand of flowering wood plants for the production of the succeeding crop.

L. H. Bailey: I would like to ask Prof. Corbett about the correlation of the vigor of the plant with the length of flower stem, with the productivity, and also the date of maturity from cuttings, whether one is later than the other, as a rule.

L. C. Corbett: There was practically no difference as far as the vegetative vigor of the two sets of plants was concerned; one was practically as vigorous as the other. Under the treatment which we gave them—they were planted in the same house and grown under substantially the same conditions—we could detect no difference in the vegetative vigor of the two sets. I think in nearly every case the flowering plants came into bloom a little earlier than the blind plants, but throughout the five years I could detect practically no difference in the vegetative vigor of the two.

The following paper was read by J. B. Norton:

IMPROVEMENT OF OATS BY BREEDING

Jesse B. Norton, Plant Breeding Laboratory, U. S. Department of Agriculture.

While cereal breeding has received a great impetus in the last few years, due to the results reached by some of our well-known plant breeders, as the Garton Bros., Vilmorin, Hays, Saunders, etc., little has been started in the line of oat improvement. For those who are interested in increasing the nitrogen content of cereals the oat promises most, for it is richer in protein than wheat or corn, hulled oats averaging about 14 per cent., while other cereals average about 11 per cent.

While there are a large number of named varieties in cultivation, it is very hard to trace the origin of most of them, many of the names being simply trade names applied to old varieties in order to meet the demand for novelties. Sometimes a good variety has been found growing in fields of other crops, and some one, noticing its good qualities, has introduced it under an appropriate name. The Potato oat is cited as an example, and also the Washington oat, introduced some years ago by Carman.

Seed growers and seedsmen have done most of the selection work that has been done with oats, and reports on the greater part of this work have never been published. In fact, the literature on the subject is very meagre, and as far as practical permanent work goes, almost no literature can be found.

The work of the Garton Bros., of England, has been written up by several scientific writers[1] and is well known to most plant breeders, but this paper would not be complete without a short review of their work on oats. Beginning work in 1880, they secured their first successful oat crosses in 1885. About 1887 they commenced to use naked oats as the female parent to avoid the mechanical difficulties in the way of crossing encountered in the hulled varieties. The Gartons have crossed and recrossed the existing types of oats until they have compound crosses containing the blood of a large number of parent varieties. Speir[2] gives the following parentage for some of the new varieties they have fixed and introduced:

Tartar King. Black Tartarian, White Tartarian, and White Canadian.

Pioneer. Black Tartarian, Scottish Potato, Waterloo, and White August.

Waverley. Scottish Potato, Naked Oat of China, White Tartarian, and Flanders Yellow.

[1]Carruthers, Wm. Cross-fertilization of Cereals. Journ. Roy. Ag. Soc., p. 684. Vol. IV (1893).

[2]Speir, John. The Produce of Old and New Varieties of Oats. Trans. Highl. Agr. Soc., Ser. 5. Vol. XII.

The work of Garton Bros. is being continued, but of recent years few reports have been published in scientific journals. Dr. William Saunders, of the Central Experimental Farm of Canada, has originated a large number of oat varieties in connection with his extensive work in breeding cereals, etc., a report of which is given in his paper before this conference. More detailed accounts can be found in the annuai reports of the work of the Experimental Farm. Rimpan[1] gives four reliable cases of natural crosses between distinct types of oats. In one of these cases the progeny was grown for several years and a number of forms selected. The parent varieties were Hallett's Canadian, a common white oat, and Black Tartarian, a black side oat. In 1879 they were plnted in two plats side by side. In 1880 a brown hulled plant was noticed in the Canadian grown from seed of the last season. In 1881 the seed of this plant produced a number of intermediate types, some of which were fixed by selection during the following seasons. Rimpan gives an excellent plate showing the varying progeny of this cross.

Thos. Jamieson[2] records in a recent volume of the Proceedings of the Agricultural Research Association of Scotland some experiments in the natural crossing of oats, but since he records his crosses as showing in the color of the hulls the same season that the cross was supposed to have been made, his results must be regarded as unreliable. But the fact that the progeny of supposed crosses varied widely the next year makes it possible that the cross might have been made the year before he started his work.

Fairchild[3] in a recent number of Experiment Station Record discusses the breeding work of the Station for Plant Breeding at Svalöf, Sweden, under the direction of Dr. Nilsson, and mentions the work being carried on with oats, but does not go into details. The work at this Station is being done entirely by selection, Dr. Nilsson finding that the natural variation gives sufficient opportunity for selection. One of the interesting experiments being carried on at this Station is the selection of pure strains of the different varieties, the separation being made on botanical characters. The selected strains show great uniformity, but are said by some who have grown them not to be so satisfactory as the original variety composed of three or more mixed strains.

The above mentioned papers include most of the scientific articles on oat breeding, but numerous popular articles can be found in agricultural journals, etc. Oat breeding and adaptation are being carried on by some of the State Experiment Stations in this country, but few published results of these experiments are to be had.

The work in oat breeding was taken up by the Plant Breeding Laboratory of the Department of Agriculture in the Spring of 1902, and though it has been carried on only one season, some of the results in crossing and experience in selection are thought to be worth recording. The literature on the subject and the past experience of the writer all tended toward the conclusion that the artificial pollination of oat flowers is very difficult and attended with a very small percentage of successes as compared with wheat.

[1]Rimpan. Kreuzungproducte landwirtschaftliche Kulturpflanzen. Landw. Jahrb. Band 20. 1891. p. 364.

[2]Jamieson, Thos. Natural Cross-fertilization and Change of Seed. Proc. Ag. Research Assoc. 1897. p 31-50.

[3]Fairchild, David G. Exp. Sta. Record. XIII. No. 9. 1902. p. 814.

In 1898 the Kansas Station made a number of crosses of oats, but later the work was given up, owing to the work on corn and wheat crowding out that on all the minor cereals. This work, in which the writer was interested, yielded only about 5 per cent. of successful crosses owing to the method followed, being similar to that which had proved successful with wheat. In this work a nearly ripe but usually unruptured anther was taken from the male parent and placed inside the emasculated and immature flower of the female parent. The pendent position of the spikelet of oats allowed the anther to drop out when the flower matured and opened without coming in contact with the receptive stigma.

Garton Bros. speak of the difficulty of obtaining successful crosses in oats, due to the difficulty of manipulation and of the removal of the anthers without injuring the delicate stigma. They claim to have obviated this difficulty by using naked or hull-less oats as the female parent. In my experiments this year the hull-less oats proved very much more difficult to manage than the ordinary type.

At the beginning of the past season (1902), before commencing crossing work in the experiments carried on by the Plant Breeding Laboratory, a study was made of the oat flower, its time of blooming, structure, etc., and the methods used in crossing were based on the knowledge acquired by this study. As a result excellent work was done, and the artificial crossing of oats can now be carried out with as great, or even greater, assurance of success as that of any other plant. With reasonable care and skill it is possible to obtain nearly 100 per cent. of successful crosses.

The spikelet of the common oat is made up of from one to three flowers, each enclosed in two tough glumes that make the hull of the ripened grain. These flowers usually fit closely together and are enclosed in two thinner and larger glumes, called empty glumes to distinguish them from the flowering glumes. Each spikelet is hung on a slender pedicel instead of being sessile on a central stem, as in wheat, barley, etc. This fact adds to the difficulty in working with oats, for each separate spikelet worked must be grasped firmly between the thumb and fingers while it is being emasculated or pollinated, and it is probable that this handling reduces the vitality of the flower considerably. The flowers in a spikelet mature on different days and vary in size, the basal one being largest and blooming first, the second flower being weaker and often much smaller and blooming one or two days later. In cases where there is a third flower it is still smaller, often being rudimentary, and blooming a day or two later than the second one. The oat flower proper is similar to that of wheat, being composed of three stamens and a one-ovuled pistil with a two-parted feathery stigma. In the wild oat (*Avena fatua*) and in many cultivated varieties the flower opens out wide, allowing the double stigma to project on each side and the anthers to fall down as far as the lengthened filaments will let them. In oats of this type it is entirely possible that natural crossing takes place, but in many of the varieties of the present day the flowers do not open wide enough to allow the stigma to protrude at all, and many flowers even keep the anthers inside the glumes. In others the flower often opens before the spikelet has pushed out beyond the sheath of the upper leaf. Again, in many varieties the anthers dehisce some hours before the flower opens. Most

persons who have worked on the problem have come to the conclusion that natural crossing in the oat is rare, but to test the point a large number of flowers were emasculated and left uncovered under favorable conditions for crossing, but no seed was obtained, while at the same time artificial crosses were averaging about 75 per cent. of successes.

The method of making artificial oat crosses during the season was in general as follows: From six to eight nearly mature spikelets were selected from the upper part of one of the heads of the desired female parent; the basal flower of each was opened, and the three anthers were removed with a fine pair of forceps, care being taken not to injure the young stigma. The remaining flowers of the spikelets were cut away and the emasculated flowers carefully closed. The neighboring spikelets on the head were removed and the emasculated flowers tied up together in strong tissue paper for protection from the omnipresent English sparrow. This paper was held on with fine copper wire, to which was attached a small marking tag bearing the date. This work was always done in the morning, at which time the anthers that would open in the afternoon could be grasped with the forceps without being ruptured.

The work of pollination is best done at from 1 to 3.30 p. m., before which time it is hard to find ripe anthers, while later than this most of the pollen has already escaped. Only fresh pollen was used. Flowers emasculated during the morning of one day were pollinated the afternoon of the following day, when practically all of the stigmas were in the best receptive condition. On pollinating, the emasculated flowers were opened and a liberal quantity of pollen dusted on the stigma from a dehiscing anther freshly removed from a selected plant of the desired male parent, and the flowers were again closed and all re-enclosed in the tissue paper. A serial number was placed on the label and under this number in the field notebook was placed all the necessary data in regard to the cross.

Dry, hot weather is fatal to good results with artificial oat pollination. Several hundred crosses made during a dry, hot spell in June yielded less than 5 per cent. of grain, while the work during a protracted cool period of weather that followed this yielded above 75 per cent. of successful crosses.

Premature pollination does not kill the stigma of the oat flower, as in some plants, neither does it prevent fecundation, as flowers emasculated and pollinated two or three days before maturity set seed.

In oat crossing the time of blooming is naturally important and must be taken into account. While with the great majority of grasses the flowers open in the morning hours, the oat flowers open only in the evening or afternoon. On an average the blooming period extends from 3.30 to 6 p. m., but under exceptional circumstances flowers will open as early as 10 a. m., while in some varieties by 2 p. m. almost all of the stamens that would ripen that day were ruptured within the flower. The flowers close again before dark, and as a rule do not open again, but some few unfertilized stigmas can be found protruding the next day. The unfertilized stigmas retain their fresh appearance for a long time, sometimes for ever a week or ten days, but rarely set seed when pollinated more than four days after maturity.

The work in selection is being carried on along the same lines as the

selection of corn and cotton, and the same general forms of blanks are used for making records. Numerous varieties were grown, and those that came nearest to the different ideals were saved for selection, while those that seemed to be inferior were discarded.

As yet the work is confined to the Arlington Experimental Farm, near Washington, but later on will be carried to other oat regions. One important problem taken up is the selection of a good winter oat, the qualities to be selected for being increased resistance to cold winters, rust resistance, increased yield, strength of straw, size of grain, reduced percentage of hull and the percentage of empty hulls, and the absence of awns. This will be done by means of the score card system used with cotton and corn. The best yielding plants, with a small percentage of rust and showing a tendency not to lodge, were selected from the nursery and kept for further planting and further selection the next year. Acre plats of a number of winter varieties have been sown for selection next season, and as the Experimental Farm is just on the border of the winter oat region, the killing out of the weak plants by cold will aid materially in the selecting of hardy plants. The individual plant is taken as the basis of selection, and large plats of single grain hills are sown for this work. The plats of the past season were planted with a six-inch space each way, but this fall they are being planted six inches apart in rows with an interval of eighteen inches between rows, affording an opportunity for close observation and also for cultivation. The individual plant system is absolutely necessary for good work in selection on account of the difference in stooling, evenness of ripening, etc. In the Virginia Gray winter plats a difference of three weeks was noticed in the ripening period of individuals planted singly, while in drilled plats it was impossible to judge earliness except by the ripening of single heads.

One of the greatest annoyances is the difficulty of procuring pure seed. Among all the samples planted last Spring but one or two were what could be called pure. Most of the varieties are made up of two or more strains varying in character. Often there are what appear to be mixtures of almost all the types of oats in one lot, sometimes to such an extent that it is difficult to tell to what the name is to be applied. Under these conditions it has been necessary to go over the lots and carefully sort out the seed for next season. All of these selections will be planted next spring, and their progeny will be selected, and in this manner pure types will be maintained for comparison and to obtain known parents for crossing.

On account of the complaint of many stablemen that most of the oats on the market contain a large percentage of empty hulls, a selection has been started for a type in which all of the flowers set seed even under adverse circumstances. The rust-proof varieties seem to offer the best type for this selection under the conditions that prevail on the Arlington Farm.

Another selection started is that of reselecting some of the new varieties originated in Europe to better fit the new conditions in this country. These varieties, so far as tested, seem to sport, or break up into different types. Garton's Tartar King was planted last spring and studied in order to find out if possible whether it was thoroughly fixed in type. The plants were practically uniform in leaf and stem characters, but varied in some cases, quite

widely in the inflorescence and grains, and seeming to revert to the parent types.

In the work of oat breeding the idea is to get oat varieties that surpass in some certain combination of qualities any other variety at present in existence, and all the methods known to breeders will be used to reach these results. The addition of new varieties without any superior merit over our present forms must be regarded as a mere waste of time.

William Saunders: I decline to accept the parentage of the Big Four oats. They originated with John A. Salisbury, of Wisconsin, and any one who has read Mr. Salisbury's work would know that they might expect anything preposterous from such a source. We have only had those oats in cultivation two years, and during that time I am quite sure we have never sent a kernel of it to anybody. We should, however, have been glad to have sent to the department, if we had known any such work was in progress, some of the results of our work during the past ten or twelve years. We have been working on the oat, and the results have been reported every year in the annual reports of our farms, which are supplied liberally to the department, to the libraries, and to all the officers who care to have them. We have been going over many of the lines that Professor Norton has indicated as those that he proposed to follow in the next few years. We have originated probably forty or fifty crosses, which have been brought down by selection to about twenty or thirty. In our crosses we have crossed the white oat and the black oat, using the Black Tartarian as one sex and the white oat for the other sex, and we have produced oats white, dun colored and black, all from the same cross; and by selection and breeding, any particular feature in connection with those varieties can be perpetuated and the oat made a distinct and permanent variety. Our experiments began about the same time as Mr. Garton's, to which reference has been made, and have been widely published, and I am really surprised that Mr. Norton has never heard of the work that has been going on in Canada, seeing that we print our publications and distribute them so profusely anywhere throughout the world, and many copies are sent to the United States. Now we have crossed the branching oat with the sided oat, and we have produced half-sided oats and sided oats and branching oats, all from the same parents. We have also crossed thin skinned oats with thick skinned oats, with the idea of trying to produce thinner skinned varieties. All these have been reported on from year to year I think for the past twelve years pretty fully, and at the same time we have carried on a large amount of work in the selection of oats. We have obtained varieties from all parts of the world, growing them side by side with these cross-fertilized forms, so that we might ascertain their actual value, and I should be very pleased to send Mr. Norton samples of any or all of these cross bred that he might like to include in his work; and I regret that I did not know that Mr. Norton was carrying on work in this line, or I should have been glad to volunteer material and information.

D. G. Fairchild: It does not seem quite fair in reviewing the really remarkable work of the Swedish investigators to overlook the, to me, very interesting contribution of Dr. Nilsson, of Sweden, who has been carrying on experiments with oats for the past ten years, and he and his predecessors have really done some very remarkable work. Of course, their publications have been in Swedish, but no mention has been made of this work of Dr. Nilsson and his predecessors. I had the pleasure of looking over his very interesting station in Sweden several years ago, shortly after the Garton brothers had inaugurated their experiments, and several points were made clear to me regarding the work which have not been brought out here in these meetings. In connection with his work on barleys, he discovered a means of very easily determining the purity of the seed as regards variety. He divided the barley up into different strains which had characters that appeared on the seeds, so that he was able not only to determine the actual purity as it is generally done by means of seeds, but he could tell whether certain botanical strains of barley were mixed in a certain sample. This had to me a very material practical value. The barley, as you know, is used almost exclusively for brewing purposes, and uniformity is as valuable a character in it as anything else. He found a correlation of characters, that is to say, correlated with certain seed characters were certain qualities

adapted specially for brewing purposes, so he was able to sift out from a sample of barley that was submitted to him not only the wheat seeds and all other classes of impurities, but to determine the percentage of mixed varieties of barley. In this way he was able, by a number of years of experimentation, to get barley remarkably constant in character both as regards its malting properties and this correlated character of the ligule which he called my attention to. And it does seem to me that any one who has an opportunity of visiting Sweden, and many of us make Sweden the place for resort in the summer time, will find Dr. Nilsson's breeding station a most remarkable and instructive example of what Swedes are doing in this line.

W. M. Hays: Having originated a good many varieties of oats, I don't know how many, and having found that basing the variety on one or just two other plants, we have no difficulty whatever with types such as have been shown us, it seems quite practicable to assume that the oat is almost entirely self-pollinated, and to breed the oats you could make hybrids and make new hybrids, basing them on one or on few other plants.

The Chair: I have read with a great deal of interest the accounts of the work done in Canada at the experimental farms to which Dr. Saunders has referred as contained in the reports of their work, and, while there has been a great deal of interest, I have been specially interested in the reports on oats. It appears to me that there may be very great improvement on the lines indicated.

The following paper by E. G. Hill was read by P. O'Mara:

ON BREEDING FLORISTS' FLOWERS

E. G. Hill, Richmond, Ind.

I have seldom taken up a subject with more reluctance than the one assigned me for this paper—not that I am unwilling to contribute to the interest of this meeting and to further the purpose of this congress, which is a most praiseworthy one; but my data are so meagre, and the results obtained so different from those aimed at, that I shrink from detailing them. I have had a good deal of experience and have persevered in spite of many disappointments in cross-fertilizing roses, carnations, chrysanthemums, geraniums and begonias, as well as other plants; but it is no exaggeration to state that out of many thousands of carefully fertilized seedlings in the classes named the percentage showing advancement over the seed parents has been very small indeed.

The hybridizer tries before effecting a cross to picture in his mind the result of a union between the varieties that he selects; for the seed parent he chooses perhaps a variety with a flower of ideal form for florists' purposes; the color also is fine, but it has the defects, it may be, of a weak constitution or an ungainly habit, or other fault; he is so desirous of perpetuating the fine form and lovely color while securing vigor of growth that he selects a strong, shapely grower with as many other good qualities as possible for the pollen parent, hoping to secure progeny as near perfection as possible; he has reason to expect an approximate realization of his pictured seedling. What are the results? Not one in a thousand, probably, shows traces of the ideal that he had in mind. Occasionally, however, a seedling plant approaching the ideal will appear among the multitude germinated, and if this fittest progeny be again selected and persistently crossed back upon the original varieties used, the chances are that the end aimed at will be realized in course of time.

I would not discourage any one who is enthusiastically expecting early results: on the contrary, I would urge him to persevere; it is not impossible that among his first efforts he may secure the ideal that he has in mind. I am inclined to think that it will be a rare stroke of good fortune, however, should this occur.

Among the rose hybridizers—and they are legion—the results realized are far from what we might seem to have the right to expect. It is some twenty-eight years since the noted variety, Catherine Mermet, was raised, and, aside from its two sports, there is certainly no tea rose to dispute its

reign so far as form, finish, growth and freedom are concerned, and yet unceasing effort has been made to procure a duplicate tea variety in either yellow or red. In the effort to breed such a variety we have been made the possessors of some fine roses, but we are still seeking for the *red* and *yellow* Mermets; and one day, I feel sure, a scarlet tea rose with the good qualities of Bridesmaid is destined to appear, and that before many years, I verily believe.

It has been my pleasure to see several thousand seedling roses peep through the soil from seed fertilized and ripened on our place, but out of this whole quantity the varieties selected as of permanent value number less than two dozen, and these are to have still further tests; a few have made their entry into commerce and have strong points of excellence.

It is quite a simple operation to fertilize a rose, and many are inclined to think this the larger part of the work; but in my own experience the thorough ripening of the seed is the difficult part of the task—maturing the seed so that the germination shall produce a perfectly healthy plant. I am inclined to the belief that imperfect germination of immature seed produces sickly or delicate plants, which shows in their liability to mildew, black-spot and kindred maladies—not that it is an hereditary trait, as many suppose and as is generally claimed; but I am led to believe that the unripeness of the seed entails vital defects upon the seedlings.

The ease with which the H. T. varieties set their pods has led hybridizers of the rose to use them largely, being almost sure of quick results so far as setting seed pods is concerned. The most noted acquisitions to this class have been the magnificent varieties of La France, Kaiserine Augusta Victoria, President Carnot, Baldwin, Prince of Bulgaria, Liberty, Mme. Jules Grolez and Mme. C. Testout.

Out of a large number of Liberty-bred seedlings the tendency is to duplicate the parent even when pollinated with so double a variety as Marquis Litta, the exception being, however, in two or three of the progeny of this celebrated rose crossed with Grus an Teplitz.

No one need feel discouraged, however, for through persistent effort on the part of many hybridizers some most excellent varieties are being given to commerce, none more marked, perhaps, than the variety Soleil d'Or, raised by M. Pernet, of Lyons; W. C. Egan is a most beautiful new climber originating with Mr. Jackson Dawson. I mention these two varieties to illustrate the point that results are being obtained here and there by persistent, enthusiastic workers.

Carnations—In the effort to improve the winter-flowering carnation through cross-fertilization the results have been very gratifying; it is safe to say that fully a thousand florists of the United States have made an effort in this line, with the result that thousands of seedling carnations have been raised in the past few years; the writer knows of one gentleman who annually germinates from six to eight thousand seedlings. Besides the large operators in this line, nearly every grower of carnations is testing his own seedlings, trying to demonstrate the hidden excellences which he is sure are only waiting development; in consequence, a marked advance in the quality, color and size of the carnation is being secured.

We owe a debt of gratitude to Frederick Dorner for having led the way into this most interesting path of floricultural advancement; the efforts made by intelligent—and fortunate—workers in this particular field have given to the public strong, long-stemmed flowers with perfect calyces, while the size has been nearly doubled in the past ten years, not to speak of the widened range of color and the free-blooming qualities. Out of the thousands of crosses made last season it would not be surprising to find one or two real additions forthcoming.

Chrysanthemums. Perhaps in no other flower has the same ratio of progress been made in the same length of time as with this particular subject. The results attained have been more than gratifying, due in great measure to the systematic and intelligent procedure of those devoting their time to its improvement. Experience has systematized the knowledge obtained in this interesting family, and we have more data from which to work than with either the rose or the carnation; the semi-double forms are no longer used as formerly, the hybridizer confining himself to the larger and perfectly double forms of flowers. The change in method secures a minimum of seed, but of the small number of seeds obtained, the greater part being large, full flowers. This law does not seem to hold in anything like the same degree with the rose and the carnation, for even when both parents are full-petalled a large proportion of singles and semi-doubles are produced. It may be that when different lines of procedure are used in these two classes we may be able to eliminate a good portion of the singles, but thus far we are unable to do this.

Begonias. Here is an unlimited field for the hybridist; the several strains and types of begonias are all—or nearly all—capable of being crossed; there seems to be no reason why we should not have beautiful flowering varieties with the ornamental foliage of the Rex section.

The writer's first attempt with begonias secured the variety Bertha McGregor, which was the result of crossing a Rex with the ornamental shrubby variety *argentea compacta*. Later on the writer secured a most interesting lot of the whorled-leaved variety of Rex by crossing the ordinary Rex with the whorled variety Countess L. Erdödy. We have always regretted the loss of this lot of seedlings, from an imported fungus growth, just when they were showing some very interesting characteristics, as many as twenty-five or thirty showing beautiful Rex markings and colorings, with the distinct single or double whorl of the leaf.

We are deeply indebted to Victor Lemoine, of Nancy, for giving us that finest of all winter decorative plants, Begonia Gloire de Lorraine; to M. Lemoine more than to any other one man are we debtors for the multitude of magnificent new varieties of plants which have come to us through his patient, persevering efforts. We believe that he has earned the right to be recognized as the most skilful hybridist that the world has yet produced.

The Chair: This is a very interesting field. We are all interested in the subject. Does any one wish to follow this paper with remarks?

G. Nicholson: Our begonias have furnished a very interesting series of hybrids with the ordinary petaled and tuberous begonias; but I believe the whole lot was entirely sterile. You simply have to start with a new tuberous begonia every time. You can't

use the product of such a flower. But I saw flowers of begonias recently in a London nursery as large as a rose; magnificent things they are. Comparatively few of them have yet been distributed. They are of very great value indeed from the horticultural standpoint. In Southern France recently I was noticing the gardens of some horticulturists. It is frequently stated that the rose La France produces hybrids, but I was assured that that was not the case; that La France is quite barren. The great rose raisers say that they have tried for years and years, and that La France is quite sterile, although it is stated in many catalogues that new varieties have resulted from La France.

W. J. Spillman: There is one very interesting consideration connected with plants of some of the kinds mentioned in this paper. The most interesting example of what I wish to speak about is perhaps the apple, but on account of the long time required to get seed of the apple, it is almost impracticable to work out the suggestion with the apple. But I will use the apple because I am more familiar with it than I am with some other plants. It is generally conceded, I think, that with almost all apples that are grown under ordinary conditions, the seed may be called hybrid seed, many times hybridized possibly. Now, if Mendel's law applies to seedling apples; if we should segregate an apple tree in order that it should be certainly close-fertilized and save the seed from that tree and plant it, it would split up into distinct types according to Mendel's law. It would be exceedingly interesting to see what types occur in the apple, and we might find something of a great deal more value in the way of new apples by that means, and perhaps fix a type even that would be propagated true to type if self-fertilized. In this paper there was nothing to indicate whether the gentleman who had performed this large number of hybridizations was working with the first generation, which no man on earth can predict, or was working with the second and third generations, which we can all predict. I would like very much to know whether or not the plants were first generation plants or second or third generation plants from close-fertilized hybrids. I believe that there is a field of work there, particularly for florists, to take these plants that are multi-hybrids, whose parents were hybrids, whose grandparents were hybrids, segregate some of them, and see what comes of them, see what types they split up into. If Mendel's law is true, they certainly will split up into types that can be definitely predicted.

The Chair: Does Mr. O'Mara know whether these roses or carnations were first or second or third generation plants that Mr. Hill describes? Presumably the first generation.

P. O'Mara: I am not quite certain that I quite catch the point. That is, were the crosses made from unnamed seedlings or from varieties already in commerce?

W. J. Spillman: No; perhaps I might explain a little more fully. If you take two distinct varieties, two varieties that propagate true to seed, and cross them, nobody can predict what the result will be in the first generation; but if you will take the seed of that hybrid and grow it, it is possible to tell what it will produce: that is the point I wanted to make. Now, here is a breeder trying to produce a given plant. He crosses two plants, and the plant which comes from that disappoints him; it is not what he wants. But if he should take the seed of that plant and plant it and grow plants from the seed of that plant, the next generation will probably greatly surprise him and not disappoint him.

P. O'Mara: I think I would be safe in saying that Mr. Hill experimented exclusively with named varieties of roses which have been so crossed and recrossed that it would take a very long tracing to find out where they started from. But it is a new line to me; I don't profess to know anything particular about the subject of hybridization; but it is a new idea to say that saving the seed of the variety so produced would be certain to produce an accurate result which could be gauged in advance. I am inclined to think that the seed saved from that variety would be just as apt to disappoint the raiser as the cross obtained by this fertilization. That is as far as I know on the subject.

The Chair: That was all definitely settled yesterday, Mr. O'Mara.

P. O'Mara: Well, it is a good thing to have something settled, to settle an old question that way. The question that Mr. Nicholson brings up is a very interesting one to me, and that is the question whether or not La France is sterile. I know that we introduced several roses, two or three, I think, raised by Mr. Hill, and he gives the parent as La France in two cases. I didn't question it. I assumed that he knew just exactly what he was saying, but it is a very interesting point, and the first time I see

Mr. Hill I certainly shall speak to him, and I will write to him and ask him if he has found that to be true. It may be that variations of climate would produce a result different from what has been the experience in another place. I am sorry that Mr. Hill is not here to answer interrogations. In reading the paper over I tried to read it and get the sense of it; I think I remember one statement in it to the effect that the carnation hybridizers had succeeded in producing flowers nearly twice the size of what they were some years ago, I think ten or fifteen or twenty years ago. I am almost inclined to challenge that statement. I think that the size which we see in the carnation to-day is partly attributable to cross breeding, and also to cultivation. Now I have in my mind the recent Thomas W. Lawson; we saw flowers of it on exhibition, and we saw flowers of it in a store window grown in the same greenhouse by the same man. Some of the flowers were probably 3 1-2 inches in diameter—a great many of the prize flowers exhibited, I think, were 3 1-2 inches in diameter; a great many of the flowers sold in the store were probably 2 1-4 to 2 1-2 inches. If the character of size was fixed by the hybridizer, I fail to see why they would not all come to that size grown under the same conditions. So that I think the statement made that the hybridizer had succeeded in doing that is attaching too much importance to the work of the hybridizer and not enough to the man who cultivates them. I doubt if you go into a flower store or into any of the stores in the city to-day where consignments are received from the various growers of the same varieties, but that you could pick out a dozen samples of Bride and Bridesmaid and American Beauty and all those and lay them side by side, and to the uninitiated they might appear to be different varieties, showing what culture will do for any particular subject. I had more to do with cut flowers twenty years ago than I have to-day. At that time I was in daily association with cut flowers, roses, carnations and others, and in looking back I think that I am safe in saying that the old Edwardsii carnation, which at that time was perhaps the biggest white on the market, would compare favorably with the largest whites now on the market in point of size, and I think that President De Graw at the time that it was at its best and well grown would compare favorably in size and productiveness with the Lizzie McGowan, which for a time was the best carnation in the field. So that I should much prefer, if Mr. Hill was here, to interrogate him on these points. Some of the older florists who were in the field twenty years ago would perhaps be better able to speak on this point than I am.

W. J. Spillman: I want to enforce the suggestion I made. I did not know whether the ordinary roses are what you might call multi-hybrids or not. Since I learn that they are, I want to just make this suggestion now, that somebody save the seed of a rose and see that this rose is close fertilized; that would be necessary to accomplish what I had in mind; then see what comes of it, and save each plant separately each year and keep a record and see what comes of it, and you will be surprised at the result. You can take my word for it if you want to.

T. V. Munson: I have one fact that may be of value to those experimenting in roses in reference to the Catharine Mermet. The expectation has been that they can get something more from Catharine Mermet than what has already come about by sporting. In passing through my grounds one day, where are a number of the Catharine Mermet plants, I came upon one upon which there was one branch producing fine pure yellow roses, as fine as Marechal Niel almost, while all the other branches upon the plant were producing the ordinary flower. I intended to mark the branch and propagate from it; I was not permitted to mark the plant, I was in a hurry at the time, and before I reached it again the flower was gone and I lost the opportunity. But the fact may incite some one yet to observe this variety and possibly get the yellow rose, which would be, I think, the best thing that could be done with that flower.

The Chair: A very interesting statement, Mr. Munson, as to bud variation from that variety. It has given us two very remarkable bud variations already. I saw Mr. Ward come in a few moments ago. Could he state whether the carnations grown, for instance, these hybrids, the beautiful collection he has here, are from the first generation of seeds after the cross was made or the second or the third?

C. W. Ward: Well, I don't think that my records are quite clear enough to enable me to state. I think some of them there would probably come under Mendel's law. Years ago I commenced hybridizing carnations, twelve years ago, and I went at it free and easy for about six years. I never knew that any such person as Mendel had been

in existence until I came on the floor yesterday. I found, though, that I had gradually evolved something similar to what I believe his theory to be. That is, I have been dividing the carnations into about twelve sections, or eight sections I think, taking the crimsons and inbreeding those, and the whites, and so on, and I have got to the point where I get reproductions; that is, in crossing pinks I get pinks, and in crossing yellows I get yellows. I suppose that I have been doing something somewhat near what Mendel was doing, although I don't think that I have saved the seed from one particular plant and repeatedly planted that seed. I haven't done that yet; I am going to try it soon, though.

The following paper was presented by L. H. Bailey:

A MEDLEY OF PUMPKINS

L. H. Bailey, Horticulturalist Cornell University, Ithaca, N. Y.

Professor Bailey's manuscript was so long that he did not read it, but gave the general results of his work in extemporaneous remarks; and he then made a running comment on the significance of the work with pumpkin-like plants and the general meaning and tendencies of the new theories that are now occupying the attention of plant breeders.

His work with cucurbitaceous plants was begun in 1887, and was continued for ten consecutive years. Its original purpose was to determine whether there is an immediate influence of pollen on the fruit, a question then under general discussion; but the work soon grew into a general line of crossing and experimenting for the purpose of producing new types of fruits that might have value to the horticulturist. More than one thousand hand-crosses were made. Notes and photographs were made of the results. In one season eight acres of land were required on which to grow the progeny of the crosses. Altogether, some twenty-five or thirty acres were employed in the work. Many more than one thousand kinds of fruit, undescribed in any literature, were produced. Nearly all of these forms are yet shown in photographs. The very magnitude of the results has prevented their publication. To show the work to advantage one hundred or more illustrations should be made. However, it is doubtful whether it is worth while to publish the results in detail, because no underlying principles were discovered. The results were very remarkable, however, because of the great number of strange forms that were produced. Some of the results are published in the author's "Plant-Breeding."

Most of the experiments were made with the races of *Cucurbita Pepo*. Crookneck, Bush Scallop, Bergen squashes, the Field pumpkin and various ornamental gourds were oftenest used as parents. There was the greatest possible diversity in the progeny, in most cases no two plants bearing the same kind of fruit. In the second and third generations part of the progeny was grown from plants again hand-crossed and part from plants that were left to themselves. In some cases the plants were inbred—that is, the flowers were fertilized with pollen from another flower on the same plant (Cucurbita is monœcious). There were no essential likenesses or unlikenesses between these various categories. Even the progeny of inbred fruits was as various as that from cross-bred fruits. In no case was there any immediate influence of pollen, or xenia. In very many cases the progeny showed marked

characters that were wholly lacking in either parent. These new characters were unusual colors, shapes, wartiness of the fruit and attributes of vine.

Hybrids of two species.—In all the work with Cucurbits, numbers of attempts were made to combine the three species, *C. Pepo, C. maxima* and *C. moschata.* It is a common notion amongst gardeners that these three species intercross interminably. All efforts, however, to combine the three species have failed, and the speaker is convinced that under common garden conditions none of these species habitually hybridize.

He became convinced, however, that it is possible to amalgamate *C. moschata* with *C. Pepo,* and a definite result was secured in this direction in 1892. The result of many pollinations was seven fruits of the following progeny:

Common ornamental gourd (*Cucurbita Pepo*) by the Japanese Crookneck (*C. moschata*).

The Fordhook bush pineapple squash (*C. Pepo*) by Butman squash (*C. maxima*).

The Connecticut field pumpkin (*C. Pepo*) by the Japanese Crookneck (*C. moschata*). Of these two fruits were secured.

Japanese Crookneck (*C. moschata*) by Red Etampes pumpkin (*C. maxima*).

Boston Marrow squash (*C. maxima*) by Green Striped Bergen bush scallop (*C. Pepo*).

Early Sugar pumpkin (*C. Pepo*) by Red Etampes pumpkin (*C. maxima*).

In all these crosses there was no immediate effect of pollen. In four of these fruits, although the fruits themselves were well grown, there were no perfect seeds. In some cases the seeds were full grown and plump, but they were empty. Only three fruits gave seeds. These were the gourd crossed by the Japanese field pumpkin and two fruits of the Field Pumpkin crossed by the Japanese Crookneck. All these were crosses between *C. Pepo* and *C. moschata.* From the Field Pumpkin crossed by the Japanese crookneck fruits, eighty-eight plants were grown. These fell into about eight types, although there were only four or five well marked forms. Most of them were like a small orange pumpkin. Some were small green pumpkins. None of them showed any influence of the staminate parent, the Japanese, except that in a few the scar of the blossom end was very large, which is usually not the case in the varieties of the pure *Cucurbita Pepo.* One of the forms simulated a bush scallop squash of light lemon color. One was striped. All of these forms had the fruit stems of *Cucurbita Pepo.*

The details of these progeny (of Field Pumpkin by Japanese Crookneck) are given in the following notes:

Nos. 1, 2, 3, 4 and 5 were much alike. They were flattish, deep orange pumpkins. No. 3 was marked somewhat green.

Nos. 6 and 7 were small, hard, smooth, rather long-stemmed forms, yellow-orange.

No. 8 was pyriform and green-striped.

No. 9 was cream-color and approached the bush scallop type. Nearest white of any. Somewhat obconic.

No. 10 was an oblong, light yellow, softish fruit.

Nos. 4 and 5 had very large blossom scars.

Nos. 1, 2 and 3 had scars about half as large as above.

Nos. 8, 9 and 10 had scars about a quarter as large.

Nos. 6 and 7 had very small blossom scars.

Crosses were made in 1893 between some of these crosses themselves, care being taken to choose the pistils and pollen from plants that bore very similar fruits. Of all these crosses only one fruit matured.

An interesting result of these experiments was the fact that squash and pumpkin flowers are nearly always infertile with pollen borne by the same vine. Over two hundred careful tests were made on this subject with more than fifty varieties of pumpkins and squashes. Out of the whole number, only seven fruits were obtained that had good seeds. In most cases the ovary failed to develop. In some cases the ovary remained alive for some days, and it enlarged to two or three times its size at anthesis; but in most cases it finally perished, beginning to die away from the blossom or pistil end. In some cases, however, the fruits matured, being to all appearances normal, but they were usually empty or produced hollow seeds. In one experiment with five varieties of *Cucurbita Pepo,* representing both summer squashes and gourds, one hundred and eighty-five flowers were hand-pollinated with pollen from the same plant. All but twenty-two of these flowers failed to develop their ovaries. These twenty-two fruits grew to full maturity and appeared to be normal squashes in every way. Some of them, however, were wholly seedless, the seeds being represented by very small, undeveloped seed-coats. In a few others the seeds appeared to be good, but when they were opened it was found that they had no embryos. Of the twenty-two fruits that came to maturity only seven bore good seeds, and even in some of these the seeds were very few. All the seeds of these seven fruits were sown for the purpose of determining what the effects of inbreeding would be. It was found, however, that the progeny was just as variable as that grown from crossed seeds. The record of the progeny of these seven fruits is as follows:

Fruit No. 1. Four vines were obtained from seeds of this fruit, with four different types, two of them being white, one yellow and one black.

Fruit No. 2. Twenty-three vines. Fifteen types very unlike, twelve being white and three yellow.

Fruit No. 3. Two vines. One type of fruit which was almost like one of the original parents.

Fruit No. 4. Thirty-two vines. Six types, differing chiefly in size and shape.

Fruit No. 5. Twenty vines. Nineteen types, of which ten were white, eight orange, one striped, and all very unlike.

Fruit No. 6. Thirteen vines. Eleven types, eight yellow, two black, one white.

Fruit No. 7. One vine.

Very unusual crossings sometimes resulted in the production of apparently good fruit. For example, a bush scallop squash crossed with

the pollen of a cucumber produced a fruit to all appearances normal, but it was empty. In some of the hybridizations between the different species, as between *Cucurbita Pepo, C. moschata* and *C. maxima,* the same result was secured (as already noted). He was not positive whether these pericarps were made to grow to their normal size through the influence of the foreign pollen, or whether there may not have been other influences at work, as there is in the case of the hothouse cucumbers, fruits of which will develop to large size without any pollen whatever. However, many tests were made by withholding pollen from the flowers, but in no case did the ovary develop to any size.

It is a common notion amongst gardeners that nearly all kinds of cucurbitaceous plants mix interminably. It is a common opinion, for example, that muskmelons are rendered insipid and worthless when cucumbers are growing in their vicinity. Close observation in the field will convince any person of the fallacy of this idea, but experiments were undertaken for the purpose of testing the matter. Muskmelon flowers were pollinated from cucumber flowers, both in the house and in the open. In one case ninety-seven muskmelon flowers of various kinds were pollinated from cucumber flowers of various kinds, but no fruits developed. Twenty-five cucumber flowers at one time were pollinated by muskmelon pollen, but only one fruit developed, and that was seedless. These experiments and others coincide with those made by other investigators, that cucumbers do not spoil melons.

General Remarks.

After giving the general results of crossing the cucurbits at some length Professor Bailey spoke of the bearing of this work on the recent discussions concerning hybridization, and also of the trend of recent evolution literature. On the surface, all the experiments with pumpkins and squashes seem to run counter to the results secured by Mendel with peas and other plants. As a matter of fact, however, the work with the squashes is not comparable with that of Mendel, since different objects were in view and different methods were employed. Mendel's work was conducted with specific differentiating characters, whereas this work with the pumpkins was concerned with the gross behavior of the plants and the gross characters of the fruits. It is possible that if the work were to be done over again, with Mendel's methods and results in view, the same laws would be found to hold with cucurbitaceous plants. However, it would be a very difficult matter to determine, because of the instability of the cucurbits, the fact that they are monœcious and that constant crossing therefore is necessary, and the fact that so many variants would need to be contrasted. The subject is far too complicated for Mendelian methods until one has thoroughly mastered the simpler forms of hybridization experiments.

The work of Mendel, so recently revived, has two very important general bearings. In the first place, it is bound to set going a new discussion in respect to hybridity; second, it will be likely to revolutionize our methods of performing hybridization experiments, and of casting up the results of them. Whether or no Mendel's rules will hold good for all plants and for all characters is not yet known. The probability is that it will not. The

very fact that Mendel chose his stock plants with such great care, selecting species which are relatively invariable, that do not intercross, and that he eliminated the weak and abnormal plants, would tend to give uniformity in the results. We are in danger of becoming partisans. Professor Bailey remarked that he neither believed nor disbelieved in Mendel's laws. He desired only to know what the truth is. He thought that future experiments should be carried on along the lines suggested by Mendel, and not for the purpose of proving or disproving his conclusions.

It has recently been said that the time is rapidly coming when we can predict the results of hybridization with certainty, and can produce new varieties of plants with almost no element of chance. This hope is far too sanguine. Mendel's laws come from a contrast and comparison of specific differentiating characters. It is not so much a contrast of plants as a contrast of single characters of those plants. In ordinary crossing it will often be impossible to secure plants that have differentiating characters. What the plant breeder wants is a plant in its entirety rather than a plant with specific attributes alone; that is to say, it may be possible to secure some character that is wanted, but with this desired character undesirable ones of other kinds may be associated. Furthermore, Mendel's results show that the offspring of hybrids are not intermediates or new kinds, but that they are controlled by the characters of one or the other of the parents, so that new forms may not arise as a result of crossing. Every plant has unknown and unrecognizable characters, attributes that we refer in a loose way to the "constitution" of the plant. Moreover, one does not know in advance what characters will become dominant and which will be recessive. In other words, Mendel's law must be applied and discovered for each kind of plant; and the probabilities are that the results will be considerably modified by the conditions under which the plants grow. Again, the uniformity in Mendel's results was secured by the average totals of a great number of plants. The individual plants often varied widely in the very characters which in the average totals were relatively invariable in behavior. Now, the starting point of a new variety must be one individual plant, and not the average of a hundred or a thousand. The general results may be predicted with some degree of certainty, but how the individual plants will stand with reference to that result will be unknown. The practical value of Mendel's work to the actual plant breeder is yet in doubt, but the value of these remarkable experiments in elucidating our notions of hybridity, and in systematizing experiments, may be beyond calculation.

Professor Bailey also spoke of the recent philosophy of De Vries and his associates. Heretofore our thought has been dominated very largely by the Darwinian principle; that is, it is supposed that great differences may come about because small differences are enlarged by means of natural or artificial selection—a variety may become more of a variety. The new notion is that the important and permanent forms of plants come about as sudden sports or jumps, and that the small individual variations are incapable of growing into large and permanent varieties by means of natural selection. De Vries does not deny the power of natural selection, but he believes that its range is limited, that it cannot give rise to species, and that it does not

contribute to permanency; as soon as selection is discontinued the form again breaks up or reverts. De Vries' theory of mutations is, in a way, a rephrasing of the old idea of sports. It differs in some essential points, however. One is in the supposition that plants mutate or sport in periods, and that in the intermediate epochs they are only making ready for another mutation period; that is to say, there are non-mutation periods and mutation periods. In the pre-mutation periods, be they long or short, the plant produces incidental individual fluctuations or variations, but the great progress in variation is made in the mutation periods. This body of belief is bound to challenge our accepted notions and our way of looking at the organic creation. This, together with Mendel's suggestions in respect to heredity, promise to awaken the liveliest discussion during the next few years. The speaker thought it probable that when these discussions shall have passed their first stage of enthusiasm we shall return to the Darwinian hypothesis, although he doubted whether we should ever hold to it so completely and so strenuously as we have in the past. We are bound to make distinctions between the kinds of varieties, classifying them either as individual fluctuations and mutations, or, from another point of view, as quantitative and qualitative. In other words, it is probable that there are varieties and varieties, and that not all of them are destined to have the same influence on the phylogeny of the race.

The general trend of the discussions at the meeting, he said, seemed to be too exclusively along the line of hybridization, as if there were no other means of breeding and improving plants.

Mr. O'Mara's remarks * were heartily seconded—the fact that good care on the part of the grower is often more important than the variety merely. Often a good variety may become a poor one, or a poor one a good one, by the exercise of skill in the growing of it. Plant breeding alone cannot improve our cultivated plants. It must be combined with all good care.

O. F. Cook: I wish to raise one question, because I think that we should give our predecessors credit for standing where they stood, in order that we may not accuse them later of holding things which they didn't hold. We had, I think, a very conspicuous instance in the case of Darwin. Darwin thought a great many things and was not nearly as sure of a great many of them as many of his successors have been and he is now frequently accused of having made mistakes which he never made, but which he is accused of having made because they were made by other persons who have taken the responsibility of representing him. I fear that this will be to a considerable extent the case with Mendel. He took the precaution, I find, for which he deserves all good credit, of saying when he announced his so-called laws, that these were things that happened with the peas which he cultivated and in his garden. He did not say that they applied to all creation, or any other part of creation. He raised the question why in two or three hundred experiments they did not all work out the same way; he freely admits, and he leaves the matter entirely open. It seems to me that it is hardly fair even to talk about Mendel's law until we have reason to believe that it is a law and that it is at least of wide application. It may turn out to be very much like many discoveries in physiology and other new sciences, which are made to apply to the cases where the original investigation was made, but that may not have any very wide application.

W. Bateson: It gives me great pleasure to listen to the paper of Prof. Bailey, which I am sure we all feel was most stimulating and enjoyable. I should like to say a few words about the application of Mendel's law to these more complicated cases such as those of the squashes and pumpkins which were made the subject of his paper. I am

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*See page 116.

sorry if any one who had heard that paper were to go away with the impression—I am sure Professor Bailey would not wish to give that impression—that because of the great complexity of the results given by crossing the squashes and pumpkins and allowing their offspring to cross and the difficulty of classifying the offspring so produced, that therefore such a case was contradictory or in any way beyond the scope of Mendel's law. There is no reason so far as I can see to suppose that. If I had time, I could give you a number of cases that we do know are not included in the scope of Mendel's law, but such a case as this, on account of the great diversity of the offspring, is no evidence whatever that Mendel's law does not apply, for the following reason: Mendel's law in its original form is dealing with a statement of the results obtained with hybridization of simple characters. For instance, you cross together the green pea and the yellow pea; the germ cells of the hybrid will form themselves purely yellow or purely green. We are dealing with single characters that were put in with the parents. But Mendel's law deals with a more complicated, and, to the practical man, far more important group of cases than that, where the parental characters that are put in are not simple, and cases in which the hybrid when it comes to form its germ cells does not form the parental characters simply, but divides those parental characters into what, for the want of a better term, we call their components. For example, the color of these squashes and pumpkins. There is not the slightest doubt that these in other cases would not follow the simple rules of Mendel's creation. For example, these colors may consist not of one simple character, but six or eight or ten or more component characters. The shapes of the squashes again in all probability consist of at least six or eight component characters. When you come to observe that each plant that you obtain can only take one of each of those components from one parent and one from another, you may have combinations of an immense number of different entities taken two together, so that the complexity reaches a degree that is always beyond the reach of experiment. We cannot infer from those facts that Mendel's law will not apply. It is simply that the enormous areas which must be under cultivation when we are dealing with such an immense number of characters make it practically impossible to draw any conclusions. One word with regard to the point of the cytological investigations that were told of previously. I am afraid there is a little difficulty there for this very reason in regard to the complexity of character; there is a little difficulty in the way of ever hoping to analyze ultimately by the microscope the characters in the way that Mendel's law teaches us to believe they might be analyzed. Because it is very true that in Ascaris and in a number of other forms referred to we have reason to believe that the chromosomes of the father plant and mother plant side by side represent blocks of parental characters, that is not enough to help us to trace out ultimately the different parental forms of gametes. To do that you would have to have particles representing each parental character, not merely the whole block of chromosomes representing the father plant and the mother plant; you would have to have fragments representing each of the constituents of the father and each of the constituents of the mother, and they would again combine in the various combinations that we must expect. Complexity itself is no bar at all to the application of Mendel's law.

With regard to one point that the last speaker made; he said that Mendel's law is possibly a thing of small range of application, or, at all events, cannot be asserted to be of universal application, and consequently it may apply to comparatively a few things. It may be of interest to those who are not perfectly acquainted with those investigations if I just briefly run over the kind of characters that have been shown to follow Mendel's law. For instance, there were Mendel's seven original characters, shape of plants and characters of seeds and pods, carried into several details; then there are the animal cases; we know that it applies to the shape of the combs of fowls, to the extra toe in several races of fowls, we know that it applies to the colors of fowls; we know that in mice it applies to the curious waltzing habit of the Japanese mouse, that character involving, we may say, almost mental attributes. We know it applies to the whole series of colors into which the whole series of rats has been broken, and to, I may say, twelve or fifteen different colors in plants. I think it is not too much to say that it applies to almost every case where the test has been possible of application.

N. L. Britton: The fruit of Cucurbita was the subject of experimentation. Even in the wild species of Cucurbita there is a very great diversity of form and size of fruits, as is well known, and I think it is possible that that might have lent difficulty.

The Chair: Professor Bailey was perhaps somewhat unfortunate in the subject of his experiments.

L. H. Bailey: I believed, and I became convinced before I came through, that I had got hold of the wrong topic. It was too large for me, and I believe that one to take up the discussion of the Cucurbits and variation through hybridization has got first to be well grounded in many simple things. And from the point of view that I occupy now I believe it is one of the last things for a man to take up to work with. I quite agree with what Mr. Bateson has said, that if I could work the thing over I might be able to discover some kind of law governing these facts, but looking it over now, I can't do so. All I know now is that I got lots of things, I don't know how.

T. V. Munson: I don't know whether I can say anything that would be of further value on this discussion, but it does occur to me that we are apt to spend a great deal of time in discussing theories without arriving at any solution. It has come to me in my own work that the matter of hybridization is entirely too extended for us to begin to establish a general law. There are some intimations of existing laws in the work, but we find that whenever we begin to discuss those so-called laws, as Mendel's, we end with a great deal of pro and con discussion without solution, without a satisfactory conclusion. It appears to me like this: that everything in the entire organic world—I might make it universal—with reference to form, is the result of environment, and in that I include the subject itself as a part, a small part, sometimes a very large part, of the environment, and what we are looking at is what has produced this result. It is a result that has partly come out of the individual under view and the effects of the surroundings upon that. Now, in working upon plants, I think we should not confine our views entirely to a biological standpoint, but that we are all the time tracing chemical influences. There is a chemical laboratory in every plant and chemical changes taking place. Each variety of fertilizer given the plant produces its effect in taking up and elaborating the substances, and in carrying on those chemical changes it evidently brings about its own result. So that we might say there is one general law. It seems to me that in every direction I have observed in my work the result has come out of a set of surroundings of the environment. And when we study the environment, the soils we are using, the moisture in the atmosphere, the temperature, all those conditions have some influence. If we undertake to make a general application of such a law or so-called law as that of Mendel, which is, I think, a very small law—laws are of different capacities; some reach very far and others but a very little distance—Mendel's law applies to pure seedlings through several generations. Suppose, instead of using pure seedlings, we continue to hybridize each generation, and we have many other ways of producing varieties, applying other influences. This, then, has a very narrow limit to the hybridizer wishing to employ all the different influences that he may see fit to employ. So that, in spending time upon discussing this one small law, or a very short reaching law, we may overlook more general laws, and we shall be more likely to reach results if we collect facts, put them where the experimenter can use them, and show the extent to which we have absolutely proven. Then, like Kepler, after a time (and I don't think that time has yet arrived to establish any great number of laws in hybridization), we can probably draw some more general laws. Then I think our most practical direction in which to work is to collect facts, strive always after something that is useful, something that is practical, and make notes of every influence, everything obtained, and on these facts will grow laws that are exceedingly valuable. It is true we want laws; we want something by which we can guide ourselves in this work; but if you try to make a law out of a mere theory which is only set up for experimentation you are wasting time.

Dr. N. L. Britton was here called to the chair.

The following paper was read by William Saunders:

RESULTS OF HYBRIDIZATION AND PLANT BREEDING IN CANADA

William Saunders, Director of the Central Experiment Farm, Ottawa, Canada.

Early in the history of fruit growing in Canada there were a few enthusiastic men who devoted much time to the production of new varieties of fruits, some of whom have left behind them, in the useful sorts they have originated, valuable legacies for future generations. This work has been carried on along many different lines.

The late Charles Arnold, of Paris, Ontario, was among the early laborers in this interesting field—indeed, I believe he was the pioneer in cross-fertilizing for the production of new fruits in Canada. He chose the grape for his first experiments, and from a cross of the Clinton with Black St. Peters he produced five new sorts, namely, Othello, Canada, Brant, Cornucopia and Autochon, the first four being black and Autochon white. In 1868 a committee was appointed by the Fruit Growers' Association of Ontario, of which the writer was a member, to visit Mr. Arnold's grounds and report on these grapes, which had then been several years under trial. These new varieties were highly spoken of and recommended for more general cultivation. In 1874 the Othello, a large black grape of good quality, was distributed for trial among the members of the Fruit Growers' Association of Ontario, and was thus widely tested over the province. Except in the most favored districts, it was found to be rather late in ripening, and it is now seldom met with in Canadian vineries. The Canada and the Brant are more widely known, and are favorably spoken of by many. All these varieties are now grown in France, and the Othello is said to be extensively cultivated in some parts of that country for wine making. Mr. Arnold was awarded a medal and diploma for his new hybrid grapes at the Centennial Exhibition in 1876, and they were specially commended.

At the same time Mr. Arnold was also doing some good work among apples. In 1873 he exhibited at the meeting of the American Pomological Society, held in Boston, Mass., eighteen varieties of cross-bred apples, all seedlings of the Northern Spy crossed with Wagener. Only one of these seedlings has found its way into general cultivation, but this one is of superior excellence, and is known under the name of Ontario. It is an apple now grown largely by some Canadian orchardists, and is found to be very valuable

for export. For thrifty growth of tree, early bearing, productiveness, good quality and long keeping it stands among the best. In 1874 a special prize was awarded to this apple by the Fruit Growers' Association of Ontario, and subsequently trees of Ontario were sent for trial to the members of the Association, and by this means the variety was very generally tested. Two others of these cross-bred apples—Arnold's Beauty and Ella—are still found in some orchards, but are not generally known.

Strawberries also claimed some of the attention of this general worker. He crossed the Wilson with Dr. Nicaisse and produced Alpha, Arnold's Pride, Maggie and Bright Ida, which were well spoken of at the time, but have, I think, all excepting Maggie since dropped out of cultivation.

He also devoted some attention to raspberries. He first crossed the old White Cap with Franconia, from which he obtained many seedlings, two of which were red. These were subsequently crossed with Marvel of Four Seasons, from which the Orange King was produced. By crossing this latter with Hornet several promising sorts were originated, including Diadem, Arnold's Red and several others. These were grown by many cultivators for some years, but are now seldom found in collections.

In 1872 Mr. Arnold is referred to as having raised a new variety of winter wheat of rare excellence, having the hardiness of some of the dark skinned sorts, with the thin, white skin of the more tender kinds. That year he exhibited this wheat at the meeting of the Ontario Agricultural Association, for which he was awarded a gold medal. He subsequently sold a portion of his stock of this grain to the United States Department of Agriculture, and it has since been grown more or less in different parts of Canada and the United States under the name of Gold Medal wheat, or Arnold's Hybrid.

He also made some crosses with corn. Using yellow as the female, he pollenized it successively with red and white, and exhibited specimens which showed these three colors clearly in the kernels.

One of Charles Arnold's most successful efforts was in connection with his work on peas. He crossed McLean's Little Gem with Champion of England and produced a very dwarf variety, an excellent bearer, producing peas of high quality. The writer had the opportunity of testing it at Mr. Arnold's home in July, 1875. He subsequently sold this pea for a satisfactory consideration to the well known seedsmen, Bliss & Sons, and it has since been in general cultivation under the name of American Wonder, and is still very highly esteemed. Mr. Arnold died in 1893, at the age of 65 years.

The late Peter C. Dempsey, of Albury, Ontario, was another of the early and persistent Canadian workers in plant breeding. His first work reported was done on the grape. He made crosses with Clinton and Golden Chasselas, Creveling and Sweetwater, Allen's Hybrid and Delaware, and Hartford and Black Hamburg. One of these latter, out of a considerable number which fruited, proved to be of superior merit, and was named Burnet. It was first exhibited in 1873 at the autumn meeting of the Fruit Growers' Association of Ontario, branches with clusters being then shown, that was the second year of its fruiting. The bunch was large and slightly shouldered; berry large, skin thin, flesh tender, almost melting; sweet, delicately flavored, and quite free from foxiness. In quality it much resembled Black Hamburg. It was sub-

sequently exhibited at many meetings of fruit growers, and was the subject of much favorable comment. When brought into general cultivation this grape was found to be rather late in ripening, and some seasons many of the berries were small and seedless. The Burnet grape is still found in many collections.

Mr. Dempsey also showed a white grape the same year—No. 18—which was regarded as very promising, and was awarded a special prize. At the time this was exhibited Mr. Dempsey stated that out of a number of seedlings of this cross of Hartford with Black Hamburg five out of every seven were white. This seems remarkable, seeing that both parents were black.

He also experimented with pears, of which he produced seventeen crosses, and from these grew a large number of seedlings. One of the latter, a cross of Bartlett fertilized with pollen of Duchess, proved to be a pear of special merit, and was introduced to general cultivation under the name of Dempsey. The tree is a healthy and vigorous grower and quite productive. The fruit is large, about the same size as Duchess; pyriform; irregular in outline; smooth; green, changing to yellow as it ripens, with a slight brown tinge when exposed to the sun. The flesh is white, fine grained, tender, with granulations about the centre like the Duchess; sweet, juicy, with a rich flavor. Season, October and November.

Among other pears produced by him was a highly flavored winter variety, the result of crossing the Duchess de Bordeaux with Josephine de Malines, but this was never introduced to general cultivation.

Mr. Dempsey did some work in apples also. He crossed the Golden Russet with the Spy, and raised, among other seedlings, the Walter and the Trenton, two highly esteemed sorts, which are found in many collections. The Walter is a handsome apple—large, round or oblong, of a yellow color overlaid with splashes and stripes of deep red. The flesh is white and the quality good. The Trenton has the appearance of an apple of the Fameuse family. In size and form it resembles the Russet. The color is more intense than Fameuse and the flavor rich.

He also did some work in gooseberries, having crossed the Houghton, Smith's Improved and Downing with English varieties, and also raised some good seedling black currants, but I am not aware that any of these have survived.

Peter C. Dempsey died in the autumn of 1891, at the age of 63. His own words, spoken of other experimenters, may well be applied to himself: "What richer legacy can a man leave to the generations which are to follow him than a fine, delicious fruit which he has originated with his own hand?"

The late W. H. Mills, of Hamilton, Ontario, also deserves a place among Canadian plant breeders. His work, as far as I can learn, was confined to the crossing of grapes. In 1874 a committee was appointed by the Fruit Growers' Association of Ontario to visit and report on Mr. Mills' seedling grapes. These were found to include some promising sorts, chiefly crosses of Rose Chasselas with Diana, Black Hamburg with Concord, and Muscat Hamburg with Creveling. Descriptions were given of a number of these new varieties, but the only one which has survived the test of years is one of the crosses between Muscat Hamburg and Concord. This grape was at first named

Sultana, but later was known as Mills. The vine is vigorous and productive; bunch large, compact and shouldered; berry black, with a thick bloom; flesh meaty, juicy, with a rich, sprightly flavor. The skin is thick and the berries adhere so firmly to the stem that a cluster can be lifted by a single berry. This grape ripens about with the Concord and is a long keeper. The Mills was introduced to general cultivation in 1888 by Elwanger & Barry, nurserymen, of Rochester, N. Y., and still holds a place among cultivated sorts.

The late William Haskins, also of Hamilton, Ontario, was another worker among grapes, and effected crosses between Hartford Prolific and Black Hamburg, Creveling and Black Hamburg, Concord and Allen's Hybrid, Oporto and Black Hamburg, Chippewa and Black Hamburg, and Rogers 15 and Delaware. Several of the seedlings raised from these crosses were regarded with favor at the time, but one only of superior merit is now in cultivation. This is known as the Abyssinia, from Creveling crossed with Black Hamburgh. The vine is hardy and a thrifty grower; bunch medium to large, and compact; berry large, black and of good quality, much resembling Creveling. It is said to ripen earlier than the Concord.

The late James Dougall, of Windsor, Ontario, raised many seedling fruits. He, however, depended chiefly, if not wholly, on selection. The Windsor cherry was one of his productions. This fruit was first exhibited by him at the summer meeting of the Fruit Growers' Association of Ontario, held at Goderich, Ontario, in 1871. It was then described as a cherry of medium size, jet black, flesh tender and luscious, quality very good. This proved to be a valuable acquisition, and is now much grown.

Mr. Dougall also did some work on gooseberries. In 1873 he exhibited a number of seedlings supposed to be crosses of English sorts with Houghton. Some of these were reported as very productive, but I do not know of any of them in cultivation now.

RECENT WORK.

Among the more valuable seedling fruits of recent origin in Canada the following are well deserving of mention:

The *McIntosh Red* apple, originated by John McIntosh, of Dundela, Ontario; an early winter sort, and in its season probably the finest apple in cultivation for dessert purposes. Ripe, November to January.

The *Russell,* which was produced in Russell County, Ontario, is an early apple of good quality. Ripe from middle of August to middle of September.

Scarlet Pippin, originated at Lyn, Leeds County, Ontario, near Brockville. A very attractive apple, highly colored, and of good quality. Season, early winter.

Canada Baldwin. Said to have originated at St. Hilaire, Quebec. A medium sized fruit of good quality. Season, midwinter.

La Victoire, originated near Grenville, Quebec. A handsome apple; medium in size, of good quality. Season, midwinter.

Swayzie Pomme Grise, originated near Niagara, Ontario. Size, under medium; yellow, with a thin russet coating; highly flavored; quality very good—one of the best dessert apples of the midwinter season.

My own efforts in cross-breeding were begun in 1868, and have been continued at intervals ever since. The work done has included experiments

with the gooseberry, red and white currant, black currant, raspberry, blackberry, grape, apple, pear, plum, cherry, Sand cherry, Japanese quince, rose and barberry. Also with different sorts of wheat, barley, oats, peas and rye, and with several species of flowers.

METHODS OF WORK.

Before referring to these in further detail reference will be made to some of the methods employed in carrying on these different lines of work.

In the breeding of plants the term cross-bred is used when referring to the crosses produced between different varieties of the same species, and the word hybrid when referring to new forms obtained by crossing such plants as are generally regarded as distinct species.

The results obtained from efforts at crossing or hybridizing depend much on the care taken in conducting the operation. In a general way, however, it is believed by many experimenters that crosses in fruit inherit their constitution largely from the female, while the quality and flavor are, it is thought, much influenced by the other sex.

The tools required in cross-breeding are few; but a steady hand has an important bearing on the success of the work. The following includes all that are needed to supplement the human hand: A pair of finely pointed forceps; some camel hair pencils; paper and gauze bags large enough to enclose the branches on which the blossoms to be worked are situated; twine for tying these bags in place, and a few wired labels to attach to the branches, on which the number of the cross or other particulars may be written.

In choosing flower buds to work on, all those which are partially open should be rejected, also those which are very immature, the aim being to work on those which are so far advanced as to be nearly ready to open. Having chosen the flowers to be operated on, remove carefullv with the finely pointed forceps the floral envelopes, calyx and corolla, without bruising or otherwise injuring the internal organs. The stamens with their anthers are then torn away, leaving the pistil or pistils exposed. When all the flowers selected have been thus prepared they are at once enclosed in a paper bag, which is tied to the branch, until pollen from the other variety to be used in the cross can be secured.

In obtaining pollen of the apple, pear, plum, cherry, strawberry, blackberry, gooseberry, etc., it can generally be had in sufficient quantities, and often in abundance, if branches well furnished with blossom buds which are just about to open are cut and placed in a vessel of water in a sunny place indoors. The anthers usually discharge their pollen in the morning, and by lightly pinching them between the finger and thumb, where the skin is usually moist enough to cause the pollen to adhere, the fertilizing powder can be seen in small patches on the surface, and, with care, can be transferred by the hand to the flowers awaiting fertilization. Where one depends on obtaining pollen from flowers outside it will often be found that bees and other insects have preceded the hybridist, and in their efforts to gather nectar from the flowers the anthers have been so knocked about that much of the pollen has been scattered. If the variety from which it is desired to obtain the pollen is later in blooming than the individual to be crossed, the opening of the blossoms may be hastened by cutting small branches well

supplied with blossom buds a few days before the pollen is needed, placing them in water and exposing them to heat and sunlight in a greenhouse.

In working with grape blossoms the pollen may be collected by holding closely under the flower clusters recently opened a piece of blue paper, when bv a sharp tap with the finger on that portion of the branch a cloud of pollen dust will be liberated, which will settle on the paper below. By repeating this operation several times the quantity of pollen on the paper may be materially increased. The caps of the grape flowers will also fall plentifully, but by carefully removing them the pollen may be seen spread on the surface of the paper below. With a camel hair pencil, slightly moistened, this pollen can be collected and easily applied to the pistils of the variety to be fertilized.

In applying the pollen from the raspberry and the blackberry it has been found better to break off the expanded blossoms, and, having removed the petals and the central bunch of pistils, carry the mutilated flower on which the fringe of the stamens alone remains, and twirl these about among the

In operating on such cereals as wheat, barley and oats the process is

many pistils in the flowers previously prepared for crossing.

much more difficult. In working with wheat the head should be selected soon after it has pushed out from the sheath. This head consists of a series of spikelets, which are arranged alternately on opposite sides of the stalk. Later each spikelet will contain from two to five kernels of wheat. In the early stages of its growth the kernels in the head are not formed, but the hollow centres which they are destined to fill are occupied by the more or less developed flowers of the plant.

In the accompanying figure, drawn from nature, we have a portion of a wheat ear from which all the spikelets but one have been removed, and on one side of this, one of the floral chambers has been opened. The outer covering of chaff has been torn off and the inner covering turned down so as to expose the flower of the wheat plant to view. This is seen to consist of three stamens, threadlike at base, with large anthers overhanging a double branched, succulent, feathery pistil.

In nature fertilization takes place within the tightly closed chaffy case which encloses the flower, where, as the anthers mature, they open and the pollen is shed on the delicate, feathery pistil below. Portions of this pollen remain attached to the surface of the pistil, and from one or more of these minute, microscopic bodies a small, threadlike growth proceeds, which pierces the soft tissue of the pistil and, gradually lengthening, soon extends to its base, where it enters the ovary, and fertilization is accomplished, followed by the rapid growth of a kernel.

When operating on wheat, to effect a cross the outer layer of chaff is torn off with a pair of finely pointed forceps and the inner coating pulled back by seizing it near the top and bending it downward, which exposes the flower. The anthers are then carefully examined, and if their condition is sufficiently advanced to offer the possibility of any of the pollen having been shed, the spikelet to which it belongs is torn off and other flowers opened, until some are found in the desired condition, with the stamens green, but almost mature. These are removed with much care, as the slightest injury to the soft and delicate pistil will cause it to wither. The flower is then

covered by replacing the inner coating of chaff in its natural position. After the removal of the stamens from a sufficient number of selected flowers all other portions of the head are torn off and rejected.

Having previously collected heads of the variety or species which is to serve as the male, flowers are sought which contain anthers fully matured and covered with pollen, when the individual flowers prepared for fertilization are opened again in succession, and the soft, feathery pistil is gently touched with

Spikelet of wheat with the glumes in front, turned down, exposing the three stamens and two-branched pistil. (Enlarged four diameters.)

one or more of the pollen-bearing anthers from the other variety until a perceptible quantity of the fertilizing powder has been applied, when the flower case is again closed. After all the flowers in a prepared head have been operated on it is wrapped in thin paper, so secured by tying as to prevent the possibility of access of other pollen. As a further precaution, the covered head is then tied to a piece of stick or bamboo cane, where it remains

untouched until harvest time, when any kernels which have been formed will be mature and may be safely gathered. Each kernel, when sown the following season, will form the starting point of a new variety.

In crossing different sorts of barley the head should be worked before it is fully out of the sheath, as natural fertilization takes place earlier with this grain than with wheat.

In cereals, the single plant grown the first year will produce heads all alike, and these will usually resemble closely the variety on which the kernel has been produced. Occasionally, however, it will, to some extent, take after the plant from which the pollen has been gathered. If the cross has been successfully made, the grain obtained from the plant of the first year's growth, when sown the next season, will usually produce several different forms, some resembling one parent and some the other, while other plants will produce heads more or less intermediate in character. After selecting the most desirable type or types from a cross, all other forms are discarded, and only those retained from year to year which are true to the type or types selected. After several seasons of careful selection the type usually becomes fairly permanent. Variations will, however, in some cases still occasionally occur, and such should be separated whenever they appear, if the new grain is to be preserved true to the chosen type.

In efforts to cross cereals many failures may be looked for, and with all the skill which trained hands can bring to bear on the work the ripened kernels are always few, compared with the number of flowers operated on. A partial record of the crossing which has been done on wheat at the Canadian Experimental Farms shows that from 1,650 flowers carefully worked only 220 kernels were obtained—about one in eight.

In all efforts at cross-fertilizing paper bags are recommended for covering the flowers, on account of their closeness of texture. Pollen grains are frequently blown about by the wind, and are in many instances so very minute that they would pass readily through the meshes of the finest gauze. With grain, the paper bags have been allowed to remain on until the close of the season; but with fruit and flowers, after the fruit or seed is so far advanced as to be beyond the possibility of further influence from pollen, the practice has been to replace the paper bag with one of fine gauze, which will give free access of air and light, and thus promote healthy growth.

GOOSEBERRY CROSSES.

In my own work the first crosses were made in 1868, and were with the gooseberry. These were made with the object of improving the size and quality of what are known as the American gooseberries, by introducing strains of some of the best English sorts, and at the same time obtain varieties less liable to gooseberry mildew (*Sphaerotheca mors-uvae*), which usually affects all the English gooseberries grown in this country so badly, in fruit and foliage, as to discourage their cultivation. Those which are known as American sorts are generally said to have sprung from the wild species, with perhaps more or less mixture of European strains. They are noted for their hardiness, productiveness and freedom from mildew. They lack, however, the size and quality of the English sorts.

Experiments were conducted in crossing gooseberries in 1868, 1870 and 1871, and several hundred seedlings were produced some of which are still in cultivation. Two of them—one named Pearl, a cross between Downing and Ashton's Seedling, and another called Red Jacket, a cross between Houghton and Warrington—are popular sorts on account of their size, productiveness and freedom from mildew, and are extensively grown both in Canada and the United States. Other promising sorts not yet in general cultivation are Ruth, Saunders, Gibb and Rideau.

The early experiments also included some attempts at crossing some of the wild sorts with cultivated forms. Trials were made with the smooth gooseberry (*Ribes oxyacanthoides*) and the prickly gooseberry (*Ribes cynosbati*). The first efforts with the wild smooth gooseberry were not successful and were not repeated; by and by hybrids were produced between the wild prickly gooseberry and the Warrington, a cultivated hairy variety of the *Ribes grossularia,* and among these there were several interesting sorts, one of which was quite smooth, another sparingly hairy and a third rather strongly hairy. This latter is still under cultivation at the Experimental Farm at Ottawa under the name of Agnes. It is an abundant bearer, of good size and fair quality. The bush has the strong upright habit of growth characteristic of the wild form from which it was derived.

GRAPES.

From 1868 to 1875 a large number of cross-bred grapes was produced by fertilizing flowers of the native and cultivated American grapes with pollen chiefly from those of European origin. During this period more than 3,000 grape flowers were pollenized, and about 400 seedlings obtained. Many of these died from exposure to cold weather and many others were discarded as they fruited for lack of quality or productiveness, and only a few have survived. One of these is perhaps worthy of mention, viz., Kensington, a yellowish green grape. This was obtained by fertilizing the Clinton, an improved form of the native frost grape (*Vitis cordifolia*) with pollen of the Buckland's Sweetwater, a variety of *Vitis vinifera,* a large, greenish white grape, grown under glass. The Clinton vine is a robust grower, very hardy, and in fruiting produces a cluster which is small to medium in size, long, narrow, very compact, and slightly shouldered. The berry is small, round and black, and quite acid. The Buckland's Sweetwater is a less vigorous grower, is tender; the berries are large, pale yellowish green, oval in form and sweet, while the bunch is large and loose. The cross resembles the Clinton in vigor of growth and hardiness of vine, also in the character of the foliage. The fruit resembles the Buckland's Sweetwater in color, form, size and looseness of cluster, and in quality it is intermediate between the parents. In the fruit of the Clinton the seeds are short, while those of the Buckland's Sweetwater are longer and less plump. In Kensington the seeds resemble in form those of the Buckland's Sweetwater.

RASPBERRIES.

The first crosses in raspberries were made in 1869, and work in this direction has been continued at intervals up to the present time. In 1870 a cultivated form of the Black Cap raspberry (*Rubus occidentalis*) known as the

Doolittle Black Cap was fertilized with pollen of the Philadelphia, a red raspberry, a form of *Rubus strigosus*. The black raspberry propagates by rooting from the pendulous tips of the branches, while the red raspberry sends up suckers from buds developed on the roots, and the roots extend under the surface to a considerable distance from the base. Twenty-four plants were raised from this cross, all of which fruited in 1873, and some of them were very prolific. In every instance the seedlings rooted from the tips, but not freely, and in two or three instances an occasional sucker was thrown up from the roots a few inches from the crown. Subsequently these plants were propagated more freely by layering in spring the canes of the previous season's growth, when they rooted at almost every joint. The fruit of the best of these hybrids was larger than that of either of the parents. In color it was intermediate, being a dark purple, with a whitish bloom. The flavor was a striking combination of the characteristics of both parents. During the following four or five years many additional crosses were made. Among others, the raspberry (*Rubus strigosus*) was crossed with the blackberry (*Rubus villosus*). Most of these efforts failed, but seeds were several times produced. Usually these did not germinate; but once or twice, when one or two of the seeds did start, the plants were weakly and died before much growth was made.

BLACK CURRANT AND GOOSEBERRY.

After this work of cross-fertilizing had been continued for a number of years the seedlings accumulated to such an extent that it was difficult to find

Cross of Black Currant ♀ with Gooseberry ♂ (One-half natural size.)

room for them, and further work was for a time suspended. After receiving the appointment of Director of Experimental Farms, in 1886, a larger field

for such work was opened, and in 1887, when I removed from London, Ontario, to Ottawa, I took with me all the surviving seedlings of promise, about 800 in all, and since then a large proportion of these has been discarded and a number of new forms produced. Among the newer forms of a specially interesting character are crosses between the black currant (*Rib s nigrum*) and a cultivated variety of the gooseberry (*Ribes grossularia*). In each instance the black currant was chosen as the female, and twenty-eight of these hybrids were produced, all very similar in character. The branches of the black currant are without thorns, while those of the gooseberry are thorny. The hybrids have the branches thornless. In the form and serration of the leaves and in the hairiness of the stem at the base they are intermediate in character. The leaves of the hybrids are odorless.

The flowers of the black currant are in bunches of from seven to twelve. In the gooseberry they are usually in pairs, but sometimes they are three in a cluster. In the hybrids they are in clusters of from four to seven. The impress of intermediate character in the hybrids is also seen in the structure of the pistil. In the black currant this is single and smooth throughout and thickened toward the tip, which is flat and blunt. In the gooseberry the pistil is longer and divided to the base, each branch slender and very hairy at base for nearly half its length, the slender branches diverging toward the tip. In the hybrids the pistil is single for about half its length or more, less hairy toward the base and divided toward the tip, with divisions divergent. The hybrids are also intermediate between the gooseberry and black currant in the time of their blooming.

These hybrids bloom freely every season, but scarcely ever set any fruit. One year three berries in all were formed, and two other years one berry only These were borne singly, like the gooseberry; were about the size of a large black currant and of a dull reddish color. The seeds of these berries were carefully saved and sown, but none of them germinated. Clusters of the flowers have been artificially fertilized with pollen from flowers on the same bush, also from flowers of the black currant and of the gooseberry, but without success.

The Gooseberry Saw-fly (*Nematus ribesii*), which is not known to eat the foliage of the black currant, feeds freely on the leaves of the hybrids, which, although raised from seeds of the black currant, are recognized by this insect as partaking of the nature of the gooseberry. The Gooseberry Mildew (*Sphaerotheca mors-uvae*) also, which is not known to affect the black currant, grows freely on the hybrids.

EXPERIMENTS WITH CEREALS.

As the summer season in many parts of Canada is comparatively short early ripening varieties of grain are desired. Hence, efforts have been made to obtain early ripening sorts from other countries, notably from the northern parts of Russia, and from India. Several early varieties of wheat have been introduced, but they have proved deficient in vigor and productiveness, and the grain has not been as good in quality as the best sorts in cultivation here. Crosses have been made of these early sorts with the Red Fife and White Fife, with the view of combining the vigor, productiveness and high quality

of the Fifes with the earliness of the other varieties. Many of these crosses ripen from three to four days earlier than the Fife wheats, and some of them have manifested great vigor and productiveness. They produce an excellent hard wheat, which makes almost, if not quite, as good flour as the much esteemed Red Fife.

In productiveness one of the crosses named Preston has taken the lead. This was produced by crossing the Red Fife with a Russian variety known as Ladoga. Ladoga is a week earlier in ripening than Red Fife. Preston is about four days earlier than Red Fife, and during a test covering six years it has given an average crop, taking the results of the tests at all the Experimental Farms, of 33 bushels 58 pounds per acre; whereas the Red Fife, sown under like conditions, during the same period gave an average of 32 bushels 30 pounds, a difference in favor of the cross-bred sort of 1 bushel 28 pounds per acre. Many other of the cross-bred sorts have also made excellent records.

In the growing of wheats there seems to be a tendency toward bearded forms. Where a bearded wheat has been used as the female and a beardless type as male a large proportion of the progeny has been bearded. Variations, however, occur in both bearded and beardless sorts; the beardless forms frequently producing bearded heads, while the bearded ones more rarely produce those which are beardless. In one cross, where both parents were beardless, several bearded sorts were produced in the second generation. The varieties will vary in the length and stiffness of the beards, and many of them vary in the color of the chaff, some in the same cross having white chaff, others red. The chaff also varies as to its smooth or downy character. Any of these variations may be made permanent by persistent selection. Spring wheats have been pollenized by winter sorts; these have all ripened when sown in the spring, but, although the plants have had vigorous foliage, they have been slow in heading and later in ripening than most other spring wheats; and as they have not been especially productive most of them have been discarded.

In breeding for earliness, the best results have been had with a wheat known as the Gehun crossed with the Onega. The Gehun wheat was obtained from a high elevation in the Himalaya Mountains. The Onega was brought from the Onega River, near Archangel, one of the most northerly wheat districts in Russia. Two of these crosses, Early Riga and Harold, have been fully a week earlier in ripening than Red Fife, but the grain is small and the crop is not heavy. In our experience, any marked advantage gained in a variety of wheat in the way of early ripening usually involves a lessening of the weight of the crop.

Some very interesting varieties have been recently originated at the Central Farm by fertilizing the Red Fife with pollen of the Polish wheat (*Triticum polonicum*). This cross was effected by Dr. C. E. Saunders, after several previous ineffectual trials, in the spring of 1900. From the cross-fertilized kernel in the Red Fife head a plant was produced which, in 1901, contrary to the usual experience, grew heads and kernels quite unlike Red Fife. The seed from this sown in 1902 sported much and gave a number of different sorts of heads. Of the plants produced no two were alike. The

object in crossing these two varieties of wheat was to try to produce a good cropping wheat having a kernel much larger than the ordinary sorts.

Barley. Very distinct hybrids have been produced between the two-rowed barley (*Hordeum distichon*) and the six-rowed (*Hordeum hexastichon.*) These are ancient types, which have long been regarded as distinct species. In the two-rowed barley the additional rows on the six-rowed form are represented by chaffy scales, lying flat on the face of the head. In the hybrids produced by using the six-rowed form as the male these chaffy scales are in some instances all filled; in others only a portion of them are filled, and the kernels are often smaller and thinner than those found in the normal position in the two-rowed head. With subsequent cultivation the relative size of the kernels generally becomes more equalized, and in some instances they become very even in size throughout. Crosses have also been made between the bearded and beardless six-rowed sorts. The two-rowed barleys stool much more freely than the six-rowed sorts; the heads, also, are longer. The main purpose in view in attempting to produce these hybrids has been to originate varieties of six-rowed barley with longer heads and an increased tendency to stooling, hoping to increase the crop thereby. Both six-rowed and two-rowed types have several times been produced from the same cross in cases where the two-rowed has been used as female, also where the six-rowed has been chosen for that purpose. Some of these new sorts have made promising records.

Oats. In oats crosses have been made between those with branching heads and those with sided heads; also between white and black oats, white and yellow, and between thin hulled and thick hulled sorts, and many intermediate forms have been produced, some of which have given excellent crops.

Wheat with Rye. Many attempts have been made at the Experimental Farm to cross wheat and rye, but without success until 1892, when a cross was effected by Mr. W. T. Macoun, a variety of winter wheat being used as female and a winter rye as male. The resulting kernel was sown in September, 1892, and while to all appearance it was a wheat kernel which was sown, the plant which grew from it had the purplish character of rye, and the heads at the time of spearing had stripes of purple on the spikelets, as in rye, and in other respects the heads closely resembled rye. Nineteen heads in all were produced on the plant, but not a kernel could be found in any of them.

Pease. The most useful and productive sorts of field pease have been crossed and a large number of new sorts originated, about 175 in all; but by careful selection and the rejection of all those of less promise the number has been greatly reduced. There are, however, 33 of these cross-bred sorts still under trial, some of which have yielded remarkably well.

The number of new varieties of cereals which have been produced and tested is more than 700. A large number of these have been rejected, mainly because they were not sufficiently productive.

VARIOUS.

Barberries. Interesting crosses have also been made by Dr. C. E. Saunders between *Berberis Thunbergii* and the common purple barberry, *Berberis*

vulgaris purpurea, in which the influence of both sexes is seen in the progeny. A number of new forms have been obtained in the second generation.

A series of new varieties of *Pyrus* have been produced by crossing *Pyrus Maulei* with a brilliant flowering, semi-double form of *Pyrus japonica.* These new crosses are intermediate in size of bush between the two parents, and those which have flowered have been found to vary much in the size and hue of the blossoms. Among these is one very handsome form, with the flowers large, semi-double, and of a brilliant scarlet color. (Plate Fig. 5.)

Sand Cherry and Plum. The Sand Cherry, *Prunus pumila,* was chosen as the starting point for another line of experimental work. Attempts were made to cross this with different varieties of cherry, without success; but a cross was effected in 1896 with a variety of the cultivated plum known as Col. Wilder, an improved form of *Prunus americana.*

The fruit of this hybrid, which has been named Rupert, is nearly round, about 13-16 of an inch in diameter. In color it is a bluish black; flesh, melting and almost sweet; flavor, delicate and agreeable. The skin is quite astringent, and somewhat disagreeable when chewed. The stone is elongated and resembles that of the plum. Ripe, September 7th. (Plate Fig. 1.)

PYRUS BACCATA AND APPLE HYBRIDS.

When the branch Experimental Farms were established in the Canadian Northwest experiments were at once begun, on a rather extensive scale, with both large and small fruits, special attention being paid to varieties of the apple. During the first eight or nine years about 200 of the hardiest sorts obtainable in Northern Europe and elsewhere of cultivated apples were thoroughly tested, but with little or no success. In 1887 seed was obtained from the Royal Botanic Gardens at St. Petersburgh of a small wild Siberian crab known as the Berried Crab (*Pyrus baccata*). Young trees raised from this seed were sent to the Western Experimental Farms at Brandon, Man., and Indian Head, N. W. T., and were found to be entirely hardy. During a test of thirteen years they have never been injured by winter, but the branches have grown from the terminal buds every season. They have fruited abundantly, but the fruit is very small—not much larger than a cherry—astringent, and sometimes bitter. The trees were dwarf in habit, of sturdy growth and thickly branched, with branches extending close to the ground. From their build they are well adapted to resist the winds to which trees are exposed on the Northwest plains.

After four or five years of experience had thoroughly established the character of this tree for extreme hardiness efforts were made to improve the size and quality of the fruit by cross-fertilizing the *Pyrus baccata* with many of the hardiest sorts of apples grown at Ottawa. This work was begun in 1894, and has since been continued along many different lines. In 1899 36 of these seedlings fruited, and five of them were of such size and quality as to justify their being propagated for more general test. Since then about 50 of these cross-breds have fruited, and the number of varieties worthy of cultivation has been considerably increased. Most of the promising sorts have been tested for two or three years at each of the Northwest Farms, and have shown no indications of tenderness. The trees which are cross-bred seem to be quite as hardy as the wild forms of *Pyrus baccata,* and there is

1. Rupert Cherry (Prunus pumila ♀ Col. Wilder Plum ♂).
2. Alberta Apple (Pyrus baccata with Haas).
3. Charles Apple. (Pyrus baccata with Tetofsky).
4. Columbia Apple (Pyrus baccata with Broad Green).
5. Cross of Pyrus Maulei ♀ with P. japonica ♂.
6. Pyrus baccata.

every reason to expect that they will prove generally hardy throughout the Northwest country.

Prince—P. baccata with Tetofsky—is one of the varieties which has fruited for the first time this season. The tree is a strong grower and very productive. Fruit, 1½ to 1⅝ inches across and 1¼ to 1⅜ inches deep. Color, bright red (much deeper in color on the side exposed to the sun), with a few paler dots and streaks. Calyx dropped in most specimens; stem, 1 to 1½ inches in length. Flesh, nearly white, juicy, sub-acid, somewhat astringent, which lessens as the fruit ripens. Of pleasant flavor; compares favorably in quality with some of the larger crab apples. Useful for apple sauce or jelly, and fairly good for eating.

Pioneer—P. baccata with Tetofsky—has fruited for the past three years. The size is from 1¼ to 1½ inches across, and about 1¼ inches deep. Color, pale yellow, sometimes with a light pink shade on cheek. Stem about an inch in length. Calyx persistent. Flesh yellowish, firm, moderately crisp and juicy; sub-acid, with a pleasant flavor, astringency very slight. Ripe from 20th to 25th of September.

Tony—P. baccata and McMahon White—fruited for the first time this year. Size, from 1½ to 1⅝ inches across, and about 1¼ to 1 5-16 inches deep. Color, greenish yellow, mostly covered with red in splashes and streaks, with many yellowish dots. Stem about ¾ of an inch long. Calyx persistent. Flesh yellowish, juicy, sprightly, sub-acid, slightly astringent, with a pleasant flavor; quality good.

Manitou—P. baccata and McMahon White. Size, 1½ inches across and 1⅜ inches deep. Rather distinctly ribbed. Color dull yellow, almost covered with bright red, becoming deep red where exposed to the sun. Flesh yellowish, juicy, sprightly, sub-acid, with a pleasant flavor; somewhat astringent. Calyx persistent. Stem 1 to 1¼ inches long.

Alberta.—Pyrus baccata, female; Haas, male. Tree a strong grower and an abundant bearer. Fruit: Size, 1 4-10 inches across, 1 2-10 inches deep; round, somewhat flattened; slightly ribbed; calyx persistent; stem about half an inch long; color, greenish yellow, with a bright red check. Flesh nearly white, juicy, slightly astringent (astringency scarcely perceptible when fruit is ripe). Quality, fair to good. Season, last week in September to middle of October.

Charles.—Pyrus baccata, female; Tetofsky, male. Tree a strong grower and a fair bearer. Fruit: Size, 1 9-10 inches across, 1 6-10 inches deep; nearly round; slightly ribbed; calyx persistent; stem rather long; color, a uniform yellow and very attractive. Flesh, solid, crisp, juicy, with a pleasant flavor, mildly acid and very slightly astringent. Ripe first week in September.

Columbia.—Pyrus baccata, female; Broad Green, male. Tree a very strong grower and a fair bearer. Fruit: Size, 1 8-10 inches across and 1 6-10 inches deep; somewhat conical; distinctly ribbed, calyx protruding and persistent; stem of medium length; color, red, with stripes and dots of a deeper shade. Flesh, yellowish, lightly streaked with red; juicy, subacid, with a pleasant flavor, slightly astringent. Season, September and October.

In one of these cross-bred apples, the Carleton, a cross of Wealthy and Pyrus baccata, the seeds are increased in size much beyond that of other

crosses. Similar modifications have been observed in other cross-bred seedlings. What changes such enlargement of the seeds may initiate can only be ascertained by following the seedlings through successive generations.

To carry out as rapidly as possible the objects in view in this line of work, which is to provide apples of useful size which will be hardy enough to thrive in all the settled parts of the Canadian Northwest, the following methods are being pursued:

First. The best of the cross-bred sorts produced are being rapidly propagated for distribution by budding and grafting on Pyrus baccata stocks.

Second. A large number of seedlings will be grown from the best of these fruits, when occasional instances will no doubt occur of variation toward the male, which is the large fruit in the cross; and as far as this takes place further improvement in size and quality may be looked for.

Third. By a series of second crosses the seedlings are receiving a further portion of the blood of the larger fruits. How far this can be carried without inducing tenderness of the trees can only be determined by experiment. The first crosses seem to be quite as hardy as the native forms of Pyrus baccata.

By following the lines indicated there is little doubt that within a very few years a number of varieties of apples will be originated possessing that hardiness and quality which will commend them to the settlers in all those portions of the Northwest country where ordinary apples under average conditions cannot be grown.

ACKNOWLEDGEMENTS.

In carrying out the work of cross-breeding at the Canadian Experimental Farms during the past twelve years I have had able assistance from several helpers. From Mr. W. T. Macoun, whose work has been chiefly in cereals; from Dr. C. E. Saunders, who has given me much help along many different lines, especially in the crosses of Pyrus baccata. I have also received valuable assistance in the cross-breeding of grain from Dr. A. P. Saunders.

Some crosses in apples were made by Professor John Craig (now of Cornell) during the years he held the position of Horticulturist at the Central Experimental Farm at Ottawa. These have not yet fruited sufficiently to permit of an opinion being formed as to their merits.

Mr. W. T. Macoun, who succeeded Mr. Craig in office, has done much work in the cross-breeding of apples and in growing trees from selected seeds of standard sorts, with the special object of producing late keeping varieties of good quality which will be hardy at Ottawa.

To the work of Mr. H. H. Groff on the Gladioli I need only make brief reference, as lovers of flowers in all parts of the continent are loud in his praises, while enjoying the beauty manifest in his new productions.

In this hasty outline of the work done in plant breeding in Canada during the past forty years I trust I have succeeded in showing that many useful products have already been originated by Canadian workers, and in presenting some evidence of a coming harvest which is full of promise.

The Chair: This comprehensive review of the work done in the North is before the conference.

W. M. Hays: I wish to remark, Mr. President, my admiration of the work that Dr. Saunders has done. He has told us in this paper something of the work that has been done in plant breeding. There are a lot of other things that he has done in just as commendable a way, and I am glad to speak a word about them.

WINE FERMENTS

A paper, illustrated by lantern slides, was read by W. B. Alwood, of the Polytechnic Institute, Blacksburg, Va.

Dr. N. L. Britton took the chair.

The following paper by W. Van Fleet, of Little Silver, N. J., was read by Secretary Barron, and supplemented by lantern slides of hybrid gladioli exhibited and explained by C. Van Brunt:

HYBRIDIZING GLADIOLUS SPECIES

By W. Van Fleet, New Jersey.

In the following notes the term "species" is necessarily used in the horticultural rather than the strict botanical sense. For many years, through the kindness of Herr Max Leichtlin, Baden Baden, Germany, and others, we have been enabled to receive newly collected Gladioli from Africa and Madagascar, often in advance of their botanical determination, and at once used them for breeding purposes. For convenience it may be well to divide these newcomers into groups according to their garden affinities with well known species, and as a further preliminary it may be stated that only Summer blooming species and varieties having corms that keep well over Winter are desired by growers in this country. We have produced hybrids between the Gandavensis or psittacinus, as well as Lemoinei or purpureo-auratus sections, and such early flowering species as tristris, vinulus, trimaculatus, cuspidatus, ramosus and Byzantinus. Some of these crosses were very pretty, but rather difficult to winter over without glass protection. Purpureo-auratus × vinulus and Gandavensis × trimaculatus bloom in August and have long-eekping corms. They increase rapidly, have attractive characteristic forms and markings, but the comparatively small size and neutral flesh tints of the blooms do not rank them among decorative Gladioli.

The largest group of new species comprises types allied to G. dracocephalus. They come under the names of Cooperi, platyphyllus and various numbered forms collected during the last six years in Swaziland, Durban and Madagascar. The most promising horticultural type came labeled from Mt. Kilima-Noscharo, in Eastern German Africa. It is a slender but healthy grower, and has a fine spike, large hooded flowers, scarlet penciled with orange outside, and clear deep yellow inside, deepening into bright orange in the throat. Seedlings of this distinct form are under way, but have not yet bloomed. Platyphyllus has immense deeply ribbed foliage, looking like a vigorous young palm, before the flower stem arises, and a large corm having a hard woody covering. The flowers are rather small, red and yellow, penciled with purple, strongly hooded, with the perianth so short that the stigma and anthers protrude, a characteristic shared by other allied unnamed species recently flowered. Hybrids with large flowered garden Gladioli have little

merit in the first generation, but improve later on. Already several hybrids of dracocephalus have been put in commerce by European breeders. They are not of a character to commend the type to beauty loving amateurs, being too narrow and hooded in form and blotchy in coloring. The best dracocephalus hybrids we have seen were sent out under the name of G. hybridus asperus by Herr Leichtlin. They are vigorous, well furnished plants, bearing 10 to 14 broad, well opened flowers on a strong spike. The colors vary from orange to cinnabar red, penciled all over in intricate patterns with deep or brownish red. They are harmonious and attractive in outline and coloring. Some of the newer species of this group evidently come from acid regions, as they bloom early and ripen up their corms with great promptness. Hybrids obtained from them often show the same tendency, and a useful class of early bloomers may yet be obtained from this progeny. A tall-growing form of G. platyphyllus from Swaziland has green blooms covered with pencilings of bluish purple. By crossing it with the best violet blues of the Lemoine section we have made a start toward a "blue" class of a very distinct aspect. This form of platyphyllus is destitute of the woody corm coatings, and is of excellent constitution, having long and attractive foliage.

The psittacinus group is of great interest as the foundation of the splendid Gandavensis strain, and through it of all the fine modern garden Gladioli. We have used psittacinus very extensively, and generally obtain brilliant red and yellow blooms, a dense long spike, and a rigid upright growth. G. igneus, decoratus, and the valuable new Quartinianus are of this type. The first and last are very late bloomers, needing the shelter of glass in late Fall to perfect the blooms. Hybridizing with selected earlier blooming garden varieties lessens this tendency and imparts much beauty of coloring to the stately habit and lusty growth of this useful group. G. sulphureus or Adlami is plainly an offshoot psittacinus. It blooms early in July and has a straight spike of medium sized clear sulphur yellow flowers, sometimes having a greenish tinge. Some growers say the sulphureus of the Dutch florists is different from Adlami, but corms procured under both names from various sources produce identical flowers. This yellow species or variety of psittacinus would appear a potential breeder for the much desired improved yellow garden sorts, yet persistent work, extending over seven years, has resulted in only two good golden yellow hybrids out of thousands of direct crosses bloomed. These are the product of Adlami × Canary bird, the latter a fine American yellow Gandavenis of rich color but crooked growth. The other seedlings all came red, often very intense, with a few creamy whites, although varieties with yellow predominating were almost exclusively used in pollinating. The two good yellows are large and fine, but of provokingly slow increase. Quartinianus hybrids, especially with the new G. cruentus section, are very promising, the tall leafy plants being furnished with large and striking blooms chiefly red and yellow.

The oppositiflorus group naturally follows, as many growers have little doubt that the original Gandavensis, known to be the parent of all our superb garden strains, was produced by a union of psittacinus with something of the oppositiflorus type, instead of with G. cardinalis, as so often claimed. We have grown many direct hybrids of psittacinus and its allies with oppositiflorus

and floribundus that appeared quite identical with Gandavensis, as we have been able to procure the type, while on the other hand repeated attempts, extending over many seasons, to hybridize cardinalis with psittacinus and its allies have uniformly failed. This is the experience of more than one European investigator, and may be taken to almost conclusively settle the matter. Oppositiflorus, with its tall growth and many-flowered spikes, often opening 18 to 24 blooms almost simultaneously, together with its delicate peach-and-white tinting, seems a most promising parent for producing fine whites and light-tinted varieties of the exhibition type, but our own profuse trials, as well as the results of many contemporary breeders', show an appalling amount of chaff to very few grains of wheat. The results of the first two generations of hybridity are almost *nil* in a decorative sense, but the third consecutive pollenization with the best modern white and very light kinds has developed some very pretty and hopeful new varieties. The looked-for high-class pure white has not come by this means, though an almost stainless oppositiflorus was used at the beginning and rigid selection since maintained. Really clear whites have appeared from psittacinus and drachocephalus, hybridized with oppositiflorus, showing very strong pollen influence, but they have little vitality and low powers of perpetuation. Floribundus appears the more promising of the two as a parent, though inclined to transmit red coloring to its seedlings. Its hybrids are more likely to bear flowers facing one way than oppositiflorus, which takes its name from the distichous or two-ranked manner in which the blooms are borne. The only other useful member of this group known to us is a new one which came labeled "narrow-leaved species from Swaziland." The corm had evidently been collected when immature, and lay dormant two years, at last producing a long spike—32 flowers—of very short and small blooms, pale lilac with feathery markings of a deeper shade. The blooms face one way and open well together. It is a very late blooming sort, but a few hybrids were secured which are now well under way. All growers of Gladioli of the Gandavensis type know there is a constant preponderance of the red varieties. The white and light colors tend to degenerate with greater or less rapidity, while the reds increase in number and maintain their vigor. So rapid and complete is the reversion in some instances as to amount to wholesale atavism. Considerable numbers of a choice Gandavensis variety have, propagated for generations in the usual manner from cormels, changed in a season so as to closely resemble the typical red and yellow Gandavensis. This seems to confirm Mendel's theory of dominant and recessive factors in all hybridizations. Taking psittacinus as the dominant, oppositiflorus acts in most instances as the recessive type, and tends rapidly to efface itself in favor of its virile partner during reproduction by seeds, and to a lesser degree during extension of a given hybrid plant by cormel or bud propagation.

Gladiolus purpureo-auratus is well known to be the foundation of the popular Lemoine and Nanceianus strains of commercial varieties and G. Papilio of the "blue" Lemoinei kinds. These latter comprise a number of attractive heliotrope and purple-blue shades in the typical hooded form of the parent. Papilio albus is a handsome slender-growing variety, reproducing itself perfectly from seed. It is very pure white in color, with a crimson purple blotch. Crossed with the best whites among the Gandavensis and Lemoinei

sections it produces a few attractive and distinct new light garden forms amid a great proportion of inferior ones. We regard it as promising and will continue work among its dilute hybrids, of which we are now approaching the fourth generation.

The species typified by G. Saundersi are of the first importance. Saundersi, in the hands of Herr Leichtlin, gave us the magnificent strain known in commerce as Childsii, still of the very highest commercial value, and the large-flowered, brilliantly-colored Nanceianus sections, produced by the Messrs. Lemoin by crossing purpureo-auratus hybrids with the new species. Leichtlin used pollen from the finest procurable Gandavensis varieties on Saundersi, and the result is a class of gigantic, richly-colored kinds mostly of red tints, with widely expanded blooms having a nodding upper segment. When the reverse cross is made, and ovules of Gandavensis fertilized with Saundersi pollen, the result is far less striking. This has been verified by many thousand personal trials. G. Leichtlini is a dwarf early-blooming species, with pretty red flowers having a yellow mottled throat. It is closely allied to Saundersi and the following species, and crosses readily with both. One would consider it a promising breeder, from the dainty aspect of its wide-open blooms, but it has in our hands proved quite disappointing. Hybrids with Gandavensis, Lemoinei and Nanceianus types, with very few exceptions, lose individuality, whether the seed or pollen is taken from the species, and are a woefully commonplace lot. Crossed with Saundersi or cruentus, however, a beautiful and vigorous progeny results, quite intermediate in either case. They are early blooming, and being sterile are wonderfully profuse in bloom. Lemoine's Glaiëuls précoces look much like some G. Leichtlini hybrids, but it is understood that sulphureus is a parent to some of them.

Cruentus is a particularly showy species, very distinct, though allied to the preceding both from the botanist's and gardener's standpoint. While vigorous and profuse in bloom, if its requirements are satisfied, it must be considered a particularly "miffy" species for general cultivation. Though known for many years it no sooner appears in a dealer's catalogue than it is taken out for want of stock. Orders for corms of this species are filled with almost anything but the true article, and much disappointment has resulted among breeders and fanciers in consequence. If healthy corms are planted in nearly pure sand, with a stratum of peat for a root run, kept fairly moist, and the plants afforded plenty of sun, they make strong, leafy plants and bloom finely, but resent any suspicion of clay, and seldom thrive in rich garden soil. My European correspondents report indifferent results from crossing cruentus with other species and garden varieties, the seedlings falling off from the parents in substance or coloring. This is our own experience in the main, but the first batch of hybridized seedlings yielded the truly magnificent variety since known as G. hybridus princeps. It came from seed of cruentus × Childsii, the childsii being, as above noted, Saundersi × Gandavensis. It is not necessary to describe Princeps further than to say it almost exactly reproduces cruentus in its scarlet-crimson coloring, with white and cream featherings in the lower segments, but the flat circular flower is expanded to six inches in diameter both ways, the plant is doubled in size in all its parts, retaining the dark green lustrous and profuse foliage, and is of a vigor of

growth and virility of increase hitherto unknown in the genus. It appears to succeed wherever tested, and can be doubtless grown anywhere and in any soil. A peculiarity of cruentus in developing its flower spikes after the first buds open is fully retained. When the spike first appears it is short and blunt, looking as if only a few blooms would develop, but growth proceeds until often 19 to 22 of these immense flowers are open, the last being about as large and perfect as the first. This progressive growth continues in water, if frequently changed, almost as perfectly as on the plant. From two to four blooms are fully expanded at the same time, thus giving a flowering period of nearly five weeks for a plot of Princeps, taking into consideration the successive side spikes and extra flowering growths sent up from strong corms. During this period, from the first of August to near the middle of September, a bed of this variety rivals in brilliancy an equal expanse of scarlet salvias.

Attempts to reproduce Princeps by repeating the original cross have always failed, but many good flowers have since resulted, some of which seem worthy of perpetuation. Some odd fawn and ash colors result when crossed with species of the psittacinus and drachocephalus types. With oppositiflorus it gives a soft pink of remarkable profusion of bloom, possibly valuable for bedding purposes. The potentialities of cruentus will not soon be exhausted, and it is likely to be frequently heard of in the future.

A few unique species remain, among which Ecklonii seems most practical. It is a little plant, growing 15 to 18 inches high, with a short spike of star-shaped flowers, dull white, profusely peppered with dark purple brown. It is delicate and likes plenty of heat, but the corms are quite large and are excellent keepers. The first hybrids with oppositiflorus and light Lemoineis yielded strong plants, with long, many-flowered spikes, running into shades of wine and light purple-brown, covered inside and out with characteristic spotting of darker tints. The best of these singular hybrids, pollenized with Princeps and large-flowered garden varieties, have developed very striking large kinds with finely-shaped blooms of various pink and wine shades, with the profuse spotting well brought out. They appear well worthy of introduction as soon as sufficient stock is secured.

G. Ludwigi is an odd species of tall and very upright growth. The leaves are strongly plicate, resembling young palm fronds, and are quite hirsute, the pubescence being most strongly marked on the flower spike. The many flowers are dull salmon-pink, small and poorly opened. They are quite ventricose in form, and very late in appearing. Crossing with cruentus, the only species we could manage to get in bloom at the same time, has improved the form, enlarged the size, brightened the color and advanced the season of the bloom, while removing most of the pubescence from the plant, which is still very upright and plicate in foliage. It is not a promising species to work from, but we hope to keep at it until real garden improvement is obtained or the successive dilute hybrids become sterile.

A most distinct and beautiful little Gladiolus species was sent us three years ago by Herr Leichtlin, whose collector found it among high cliffs in a little-known part of Madagascar. It is not larger than a Freesia in growth, and produces good-sized and elegantly formed blooms of pure bright yellow. The yellow is as good as the best Jonquil or trumpet Narcissus, and not the

pallid greenish tinge usually found in the genus. It is a Winter bloomer, and our best efforts have been made to switch it around to get Summer flowers, so as to connect with yellow garden kinds. The only species we have had an opportunity to cross it with its igneus, of the psittacinus group. One viable seed was secured, which has just produced a blooming plant. No yellow coloring appeared, the wide-open blooms being quite clear salmon. It is attractive in make-up, and may be of future service, though devoid of decorative value. If the yellow Madagascar species is ever obtained in quantity it will prove a treasure for Winter blooming. The little corms resemble those of Summer-blooming kinds, and are good keepers.

It might be supposed that during 16 years of active hybridization among Gladiolus species, resulting in over 150,000 seedlings, many commercial varieties would be produced. Although we have found beautiful and promising novelties in this mass of hybrids and variety-crosses, only two so far have been thought worthy of naming and commercial introduction. One is the Cruentus-Saundersi-Gandavensis hybrid, above mentioned as Princeps. The other is a direct cross between purpureo-auratus and Saundersi, known in a limited way in the trade as Lord Fairfax. It produces a long, curving spike of Indian-red bloom, with a yellow and purple spotted throat. These are often five to seven inches across, and look like Hippeastrum blooms arranged in a Lily-of-the-Valley manner.

The Chair: I am sure the thanks of the meeting are due Mr. Van Brunt for this very beautiful display. Is there any discussion of the subject?

H. F. Roberts: Mr. Groff himself, to whom Mr. Van Brunt referred, is a member of this conference, and is present with us this evening. He had the kindness to communicate some very interesting facts to a few of us, and especially facts of interest to plant breeders. Perhaps this evening he would communicate them also to this audience and permit them to ask him questions. Perhaps he might speak especially of the case of the hybrid of the Morning Star.

H. H. Groff: It was only a moment before the presentation of this paper that I had any intimation that I was supposed to make the lightest reference to my work here to-night, otherwise I would have had great pleasure in at least making some notes in order that I might give you something of the result of my experience rather than probably what you would consider a few disjointed remarks at the present moment. Before saying anything further, I wish to pay tribute to Mr. and Mrs. Van Brunt for the excellent artistic work that they have done in the reproduction of my hybrids. I wish to say further that it is a matter of regret to me that I notice very few of those among them that I consider even the first class types of my productions, and, further, that I would have had great pleasure, if it had been possible, to see that such types had been placed at their disposal for reproduction here to-night. I must make a rather egotistical statement; from my own experience in testing the very best productions of the world's greatest and oldest workers, many of these lead in a very marked degree anything obtbainable commercially. Before making any further reference to my own work, I also desire to pay a tribute to the excellent scientific work of my friend, Dr. Van Fleet. Dr. Van Fleet is the most experienced worker on scientific lines in connection with the species of any worker or breeder of the Gladiolus in America, and I am sorry that he is not here to-night to speak for himself in regard to many very valuable points of interest which he might explain to you in regard to the first crosses. As you have heard me say in the course of our meetings, there are two kinds of workers in the field of hybridization: the one who works with the idea of finding out the why and the how and wherefore scientifically, and the other for the purpose of producing definite results of a certain character. I may class Dr. Van Fleet among the former, and I wish to take a place among the latter, with the ideal of producing high-class economic types for decoration and of appraised value for perpetuation and multiplication. That is one of the great characteristics

of my work, great vitality, rapid multiplication and reproduction. You may well understand that it is very, very important for the worker with hybrid forms, having produced a variety, that it should possess sufficient vitality and stability, not only to maintain satisfactory conditions at home, but also to reproduce itself in any changed conditions of soil and climate in any parts of the world to which it may be sent. This is one of the most vexatious questions with which we have to deal with regard to hybrid values. Comparatively few are capable of reproducing their excellent qualities under the many changed conditions, the result of the great revival of horticultural activity all over the whole world. Professor Roberts has made some reference with regard to one of the questions of variation. These questions are of great scientific interest to him as well as to other workers, and further, I have stated in conversation with my friends that the Gladiolus, having been hybridized to at least as great an extent as, if not greater than, almost any other plant form in the floral world, gives many object lessons which would be of infinite value to those workers in all the branches of hybridity. The case of sporting, so to speak, that I wish to refer to, was a named variety of Lemoine's. I had grown it for several years, and increased it by purchase year after year from the originator. It was a scarlet, with many beautiful markings. After growing it for a year or two, one day I happened to notice that the upper petal and the two lateral petals from the midrib on one plant had changed to cerise. Knowing that this change was entirely different from such combinations as I had before, that is, one side red flowers and the other side white—in a case of that kind I would expect that when that bud came to divide we would have a bud producing a red stripe and one a white stripe, but in this you will see that the variation was a cross in the flower—I marked it out, kept it over to the next year, and the whole ground color of the flower took on the cerise color. I have multiplied that from the corms year after year, and it has still held true to its sporting condition. I have also bred it, so to speak, with light colors and have been able to produce lighter shades of the new color from this parent which may be very attractive.

There has been in times past some reference to the fact that I have not made public a great deal of the detail in connection with this work. I may say that the only reason for that is that I have never been asked, and further, that the longer I work the less I feel that I have to say. However, I may say that my friend, Mr. Barron, in commenting on my exhibit at the Pan-American, which many of you of course saw, characterized the exhibit as "Mr. Groff's much mixed hybrids." I acknowledged the title and accepted it, and I told him that I thought it was a fitting term for the greatest nation the earth had known, the American nation.

The following paper was read by C. W. Ward, and accompanied by lantern slide illustrations.

THE IMPROVEMENT OF CARNATIONS

By C. W. Ward, Queens, N. Y.

I feel somewhat delicate about describing my experiments in breeding carnations, inasmuch as what has been said upon this floor during the past two days has convinced me that the bulk of my work has been done purely in an experimental way, groping, as it were, in the dark, with but little knowledge of the subject.

Until yesterday I knew nothing of Mendel's theory and laws; in fact, did not know that such a man as Mendel existed, much less that he had discovered anything germane to the interesting subject of hybridization. However, after listening to the most interesting descriptions that have taken place here, it would seem to me that to some extent I have been dimly following Mendel's theory during the past six years. I commenced the breeding of carnations twelve years since. The first six years were spent in indiscriminate crossing. always avoiding interbreeding because of the conclusion formed that it tended to weaken the stock. I must confess that during these six years very little progressive advancement was made, although a number of good commercial carnations were produced. Six years ago I determined to work according to a definite system, and to breed from definite shades of color and upon defined habits, using plants that produced perfect calyxes. After securing desired habit and color, my plan was to afterward get as large blooms as possible. In this I am at present apparently succeeding, and for the past four years the results seem to be progressively improving.

I have divided the subjects for my work into nine classes, or sections, as follows:

The Crimson Section, comprising the varieties General Maceo, General Gomez, Harry Fenn, Governor and President Roosevelt, which section now seems to some extent fixed, the greater percentage of the seedlings producing crimson blooms of fairly well defined characteristics.

The Dark Pink Section is also fairly well established, as also are the Scarlet and White Sections; while the Light Pink Section is beginning to show signs of reproducing its kind.

The Yellow and White Variegated Sections, as well as the Fancy Section, are not as well fixed, and I believe they will be very difficult to fix, as they are hybrids in the true sense of the word, being made up of characteristics common to several different types, and we must always expect in them a considerable variation.

The Blue Section has proven the most difficult of all, as the seedlings sport backward and forward between light lavenders and deep purples, the intermediate colors being largely a composition of pink and blue, some of them curiously and brilliantly colored. As yet I have not succeeded in securing a true blue, the nearest thing being a lavender colored flower, in which, however, there was too much pink.

In several crosses a number of curious seed sports have been produced, frequently a cross between two crimsons producing pure white blooms, and sometimes a delicate daybreak pink, the latter color invariably being slightly variegated with a crimson or purple streak, and frequently with picotee edges of the same color. This occurs where, as far as my records show, there has been no white or pink blood in either of the ancestors, and is probably due to the union of two like recessive characters existing in the ancestry, which have become dominant in the individual which we term the "seed sport," My studies of these variations in color seem to show that while there may be several intervening hybrids that do not show the dominant character of the sport, the character must, however, have prevailed to some extent at some period in either one or both parents.

In carrying on my work not less than 50,000 hybridized seedlings have been grown, and while I consider the first six years practically lost, I have been pleased to note that the advancement has been rapid during the latter six years. During this period thirty-six varieties of commercial carnations have been produced and profitably grown at The Cottage Gardens. Of these sixteen have been introduced to commerce, four being crimsons, two scarlets, two yellows, two variegated with white ground, three whites and three pinks. There are now in my collection twenty hybrids that are commercially valuable varieties, divided as follows: Four crimsons, two scarlets, two yellows, two white variegated, four pure whites, four dark pinks and two light pinks.

My most successful work in the fixing of colors was accomplished with the Crimson Section. It is now fairly well established as a type, and is more constant than any of the other types, and I have noted that many other American hybridizers are using Maceo, Gomez and Governor Roosevelt in producing their crimson sorts. The progeny of the above varieties have broken into two different colors—crimson and scarlet. This may be explained by the fact that they were derived from Mr. Dorner's Meteor, a good crimson, and the English variety Winter Cheer, which is a true crimson scarlet. The modified habit of Maceo, which I consider the most profitable commercial habit in existence, is being generally bred into the other sections, and in the end I hope to produce most of the desirable colors in carnations upon this habit.

I have found, as a rule, varieties do the best near the localities where they originated, or in similar environments. Those varieties produced in the East have, as a rule, done better with us than those produced in the West. Inasmuch as we are working with a purely hybrid plant, the result of several hundred years of hybridization, we must expect that the recessive characteristics described by Mendel will be constantly reappearing in the way of bud sports. This is well exemplified by the various sports which have occurred in the variety "Mrs. Lawson." I have known of several pure white sports, prob-

ably as many variegated, and one a dull scarlet. If we continue our present method of somewhat haphazard breeding we will, no doubt, not only have more of these sports in the future, but we will have varieties which will show great variability in many important characteristics, and possibly the cultivation of the hybrid carnation may become increasingly difficult in proportion to the carelessness with which we breed them.

I am of the opinion that indiscriminate breeding on a large scale, while it may produce occasional good results, is largely a waste of effort, and I would heartily recommend breeding to secure a definite improvement. I have always kept fairly accurate records of the work done, and have proved to my own satisfaction that more uniformly better results were secured from the pedigree stock than from the ordinary indiscriminately bred carnations.

As to the determining influences of the respective parent plants I have not been able to bring myself to believe that the pollen parent has a positive determining influence upon color, but where the desired color predominates in several generations of both parents it is comparatively easy to reproduce that color. I have also not been able to decide that the pistilate parent has a definite determining influence upon habit, but believe that the same rule will hold good as that mentioned before as applying to color.

As before mentioned, in working for a definite result several intervening hybrids bearing but little resemblance to the type sought may occur. But, if the type sought predominates in the ancestry of both parents, the chances to secure it in an improved form will be increased. In my opinion the combination of better culture with the improved hybrids has in a large measure secured our present improvement in carnations. I mention this particularly as I well remember the time when many of the older growers of carnations found fault with the modern hybridizer because he grew his carnations in rich soil or practised feeding the plants in order to secure better results. Much may be done to aid the improvement of the carnation by selection of propagating wood. After the hybrid has been secured it can be much improved by a skillful selection of the cuttings used to perpetuate the variety. This seems to be proved by the fact that we have been able to produce a fine commercial variety by selecting the strongest and best cuttings from an inferior hybrid plant, and continuing this selection until we secured a fine improved habit.

During the course of my experiments I have become convinced that the condition of the plant at the time fertilization takes place has an important influence on the progeny. This has led me to do my fertilizing, as far as possible, when the plants are in the strongest and most vigorous condition. I do not mean to say that the plant should be highly fed, as where very high feeding is practised or the soil is overly rich it is very often difficult to get seed. But I particularly desire my plants to be growing in the best possible atmospheric condition, and in a soil that produces a vigorous yet normal growth. I should not expect the best seedling carnations to be produced by plants growing under unfavorable conditions, but quite the contrary. The elimination of undesirable individual seedlings is practised to some extent before setting the plants in the field, this being determined by judging the plant for habit alone.

Each year I find several good varieties in each class, and the question of

commercial sorts seems now to have resolved itself into selecting the most desirable variety in its class.

At The Cottage Gardens we are working with a type of carnation distinctively of American origin, the only recent infusion of foreign blood being Winter Cheer, which occurred about the year 1894. Some years since Mr. Dorner and myself used to exchange varieties, but about four years ago Mr. Dorner became of the opinion that little was to be gained by the exchange, and desired to keep the results of his work, fearing that I might become too much of a competitor if allowed to use his hybrids, and the exchanges were discontinued, but the results which I have obtained since these exchanges were stopped have been markedly better than before, and perhaps since that time I have been unknowingly following Mendel's theory much closer. As stated before, I have known nothing of Mendel's theory or law until the day before yesterday; but what I have heard here regarding Mendel has awakened an increasing interest in the work of hybridizing, and I shall secure his books and read them with the greatest interest, for if there is a fixed rule by which I can produce six inch carnations on four foot stems I certainly wish to learn that rule.

H. H. Groff: I think the cause of Mr. Ward's failure was that he did not know the composition of the hybrid from the seed of Mr. Dorner, so that when Mr. Dorner sent him a light one he had no idea, of course, as to what had produced it. In regard to the matter of producing light types from dark species, my experience reminds me of a point, probably the crucial point, of what might be called my system, for the improvement of like types by crossing them with the blood of other types. By way of example, let me say that I took 100 plants, giving a crop of 500 flowers of a certain kind, of European origin, which possessed satisfactory vitality; at the same time, the quality of the flower was not up to my standard, and the idea occurred to me of producing better varieties of white—the demand being greater for that class of flowers than for any other. I proceeded to revitalize that with selected types, preferably of blue and yellow. The result of those crosses is some of the finest light color that I have ever seen, satisfactory in every way, large, strong, vigorous and beautiful. Further, if I desire to produce a certain line of types from any existing either classified or unclassified species, my first work is to produce a satisfactory type from that species and to use that type as the foundation for a new strain or family, as well as a revitalizer for existing types. Now, if that species happens to be a red one, and I am using a strong white bred from that, it is quite natural that among my white crosses I get a great many reds. I expect that, and many of these are reds of high color. At the same time, the effect of crosses of that kind upon the light types is a great improvement in size, form, color, quality, disease resisting qualities and so on, until this year I had the satisfaction of seeing my light hybrids grow seven feet high from the ground. Of course, you have a great many discards, but at the same time it is worth while.

C. W. Ward: In reply to Mr. Groff, I might state that in seeking for habit I have been following this system, my present system, for color only. The most valuable characteristic that a carnation can have is color. If it has a pleasant color it will sell; size, form, and everything else fades into insignificance beside color; color is the great thing. Now, I made up my mind that if I could secure the proper shades of color, the matter of size and habit would come afterwards, and I found that to be true. Now I have gone so far in the revitalizing of my strains as to go back and use single flowers, that is, taking the single flower for the seed-bearer. Of course, in hybridizing carnations we are working for double flowers and large sized flowers. Mayor Grant, which was thrown upon the screen, was a seedling from a single scarlet flower, which was a descendant from an old variety named Portia, a small variety but a strong grower. We obtained the Maceo from the Mayor Grant, and that is the intermediate flower; that is, it is the flower from which we get the Governor Roosevelt and the other crimsons. Now, you will find in working carnations—at least I have found—I have done a little work in geraniums and a

little work in melons, and in the melon work my experience was about the same as that of Professor Bailey—but I found in working carnations that where you inbreed, say you cross crimsons, take Maceo and fertilize Maceo, and you follow that up to the extent of making the foliage almost crimson, as soon as you got to that point there wasn't anything left in the constitution of the plant; it was run out, in other words. I didn't get any seed to speak of from it, and what seedlings I did get were runts. Now, I exercise a certain amount of selection in my seedlings. That is, after they show the fourth or fifth leaf, if the habit of the little seedling doesn't suit me I pull it up and throw it away. Seedlings of a certain habit are of no value; and after three or four years of work I found that out, so we don't waste any more work on them. But in carnations I found it nececessary, where working within very close lines, to introduce influences from the outside; that is I take, say, a single crimson with a strong habit, and I will breed into that single flower in order to get strong habit, and then go on with that; and I do the same with whites and scarlets, and all the way through.

H. H. Groff: Allow me to report an experience with a double or semi-double type of gladiolus, the flowers of which when separated and placed in a shallow vessel of water resemble the Nymphaea. Usually that blood in many of my crosses has resulted in the preparation of seeds producing twins, and in one case, in order to prove the question of these being identical, I kept the first ones separate and found that they produced identical flowers. Since then, my last season results show the largest number of twins from single seeds that I have ever produced within my experience.

C. W. Ward: I have heard that some hybridizers advocate keeping no records. I have religiously kept a record of every cross I made since 1894. Now I find those records valuable in this way: I don't study them very much when I make my crosses, but after I have accomplished a resu - I can go back and find out how I did it, and there is the most value. Now I can trace every single crimson flower there back to Maceo or Gomez; and when it comes to the whites or any particular stripe, in tracing the pedigree you will find that from the pedigree in both parents the color that you have had exists somewhere back there. Now, when you find that you make a cross, for instance, between two yellows and you have got ten scarlets and three yellows, and if you go back you will find that the scarlet has been the predominating color in the pedigree on both sides. So that in just studying my pedigrees I have come to the conclusion that the closer you breed to a color the surer you are of getting that color; and that also holds good for fragrance.

H. H. Groff: I also kept notes until after the production of over 5,000 desirable types, and it became rather burdensome.

C. W. Ward: I also produced a large number of desirable forms, but then I picked the best of the desirable forms.

W. Bateson: I would like to know whether in breeding from the light sections you ever produced dark seed. Breeding from the dark sections you get to the light, but breeding from light sections, question, do the darks ever come?

C. W. Ward: In pinks they do; we get dark pinks. But I never got any crimsons except in dark pinks where the crimson existed in the pedigree. I presume that this Daybreak pink that I get in the crimson comes in from Mr. Dorner's work; I can't trace it in mine.

W. J. Spillman: Careful work of this kind is of immense value to the scientist, and I can't help saying either that when these practical men, who occasionally rather speak slightingly of theoretical men, learn the immense value in dollars and cents of knowing the why and the wherefore, and how to do it again, it will be worth a great deal to them. In other words, if Mendel's law is true, it is worth millions of dollars to the breeders of plants in this country. If it is not true, it is vastly important that we should know it soon.

President James Wood took the chair.
The following paper was read by N. E. Hansen.

THE BREEDING OF NATIVE NORTHWESTERN FRUITS

By N. E. Hansen, Horticulturist, Brookings, S. D.

All who are familiar with the climate and soil of the prairie Northwest, and with the history of the fruit culture of this vast region, know the practical importance of this line of work. We must create a new pomology. Almost all the varieties familiar to Eastern fruit growers are tender and worthless on the open prairies of a large part of the Dakotas, Minnesota, Northern Iowa and the Canadian Northwest. With a view to meet this demand for hardier fruits the writer has engaged extensively in the work of originating news sorts better adapted to the conditions. At present considerably over one hundred thousand fruit seedlings are on the grounds of the South Dakota Experiment Station as the result of this determination. The wild fruits of the prairie Northwest form the main material, although some work is being done with the apple. The methods pursued are simple, being mainly the carrying out of the principle laid down by Darwin, "Excess of food causes variation." This is the star to which the wagon is hitched. Crossing and hybridizing are used as means of hastening the process of evolution by introducing new elements of variation. The chief reliance is placed on selection from large numbers. Some of the crossing is done under glass to guard against undue loss from unfavorable weather conditions at the time of blossoming. One of the main lines of work is the improvement of the native sand cherry (*Prunus Besseyi*). Over five thousand seedlings formed the material for selection in the first generation, and over fifteen thousand in the second generation, most of which bore the past season. Some of the seedlings bore fruit measuring fully three-fourths of an inch in diameter and of good quality, and this the third season from seed. My fall inventory will not be complete until digging time, but several thousand seedlings of the third generation have been raised this year. Over seventy-five varieties have been selected as being worthy of propagation. These are being budded on native plum roots, and an effort will be made to breed them true to seed. Some extra large native seedling plums bore heavily this year. Also a few raspberry seedlings of half wild and half tame ancestry were selected. Some two hundred strawberries, also crosses between wild and tame, were selected from some eight thousand plants. The strawberries as brought from the Eastern States are not fully hardy in the northern part of South Dakota, and a hardier strawberry is much needed. Some interesting results appear; for

instance, the everbearing strawberries as imported from France winter-killed, but their hybrids with wild Dakota strawberries proved hardy.

The field is a wide one and the demand for hardy fruits urgent. The practical importance is self-evident, since fruit culture is essential to true home-making upon the open prairies.

RESOLUTION.

Dr. N. L. Britton presented and moved the adoption of a resolution commending the movement inaugurated by the American Association of Agricultural Colleges and Experiment Stations toward a general co-operation in the study of heredity and the improvement of plants and animals. The motion was seconded by Mr. Watrous, and carried.

Upon the suggestion of Mr. W. H. Evans, the secretary was directed to transmit a notice of the action just taken to the secretary of the American Association of Agricultural Colleges and Experiment Stations, and also to send copies of this resolution to the horticultural press in advance of the publication of the proceedings of this conference.

The following paper was read by T. V. Munson.

ADVANTAGES OF CONJOINT SELECTION AND HYBRIDIZATION, AND LIMITS OF USEFULNESS IN HYBRIDIZATION AMONG GRAPES

By T. V. Munson, Denison, Tex.

In this paper no attempt is made to prove its main propositions, which are put forth in dogmatic form rather than inductive, in order to be brief, referring to Bulletin 56 of the Texas Experiment Station in part for proof and to the numerous seedling and hybrid grapes produced in the United States and France in recent years, in which the parentage is known and published in the various works upon grapes as further proof and illustration. I also respectfully invite your attention to the photographs of species and varieties of grapes I have placed in your exhibition hall. [This comprehensive collection of photographs was afterwards presented to the New York Botanical Garden.—Ed.]

Before we can well proceed to present the subject we should have in mind the object sought in improving grapes, which is, primarily, to get the best possible fruit in greatest abundance. This includes two classes of special considerations, namely:

(*a*) To better the vine—

1. In length of life { by better resisting disease. / by better enduring climatic extremes. / by better adaptation to soils.
2. In productiveness { by sure and abundant setting of crop. / by capability of fully and evenly ripening. / by persistence of berry to pedicel.

(*b*) To better the fruit—

1. In size of cluster and berry.
2. In color and prunose bloom.
3. In texture and quality of skin and pulp—
   (*a*) for table.
   (*b*) for wine.
4. In number, size and freedom of seeds.
5. In handling, carrying and keeping qualities.

How may this complex object be obtained in the fullest degree?

By selection alone? No; for it would require an immense number of successive generations from an original parent to reach any great degree of excellence in even a few of the above mentioned requirements, and that the process of selection, alone, without crossing or hybridizing, becomes in and in breeding, which invariably weakens vitality and lessens productiveness, as seen in the Martha, Lady and many other pure seedling descendants of Con-

cord in the third, fourth, fifth and later generations. Besides no new distinct flavors or other characteristics, not already bound up in the original parent, can ever be obtained. All pure progeny of Concord are essentially Concord in character, the chief variation being in color, ranging from black to greenish, or yellowish, dull white; never red. This tendency to albinos indicates weakness in vitality. The best developed seedlings, by pure selection in any one particular point, would rarely ever be selected to continue the breeding, for, in general, such varieties are found to be ruinously deficient in having other good characteristics retained sufficiently to make them profitable. This single-line endeavor to get perfection out of imperfection is similar to trying to pour two gallons of wine out of a one gallon jug. But by pouring into a two gallon jug a gallon each from two one gallon jugs, each having a different kind of wine, two gallons of a different and possibly a better wine than either that entered into the blend can be poured out again. This, told in a figurative way, is what crossing and hybridizing may do.

But will indiscriminate crossing and hybridizing, without selection, make any progress? No; except that it would produce forms that could never occur by selection alone. This often occurs in wild nature, and in many cases it is a step backward, so far as being beneficial to man is concerned, as seen in the crossing of popcorn and field or sweet corn, the mixing of pumpkins and squashes, etc.

Then, will conjoint selection and hybridization serve a better purpose? Yes, most assuredly. How? Why? Just how, we cannot tell fully. If the process of intermingling varietal and specific characteristics were purely only a mixture we could better explain, perhaps, but there is something more than mechanical mixture. Subtile chemical changes take place, and new organic compounds are formed under the stimulus, and in the union, of specific bloods. New flavors, not found in either parent, come forth; new colors arise, new forms are built up, so much so, that often botanists would pronounce them new species, if found in the woods. Yet the critical, trained, analytical eye discovers the characteristics of each parent in the hybrid. The tongue discovers the two specific flavors; generally, the eye sees the blending of specific colors. The mystery of two becoming "one flesh" the scientist may never unfold, but the wonderful fact remains for practical benefit. Further on, more of the "how" will be given.

Why conjoint selection and hybridization serve a better purpose than selection alone, we may be better able to answer.

The very fact that crossing, or hybridizing, enables us to embody in a variety flavors, colors, seasons, chemical properties, vine characteristics of resistance to disease, adaptation to soils and climates, that selection alone could never reach, is one of the best reasons for employing the conjoint methods of development.

The immensely greater rapidity and extent of progress the conjoint method permits is another very important reason why it should be employed wherever available.

It may be clearly seen by a keen-eyed student of nature, as it is quickly learned by a practical originator, that to progress much in all the points . apped in our scheme for the best development of grapes (and these prin-

ciples, as illustrated among the species of Prunus, will apply quite as well in any genus of plants having several species, as with *Vitis*), it is very essential to employ some rapidly accumulative methods which will bring together in harmony and stableness the information sought.

The process might still further be speeded, probably, by uniting in it budding and grafting, which Professor Lucien Daniel of France claims will, in many cases alone, produce true hybrids—as between tomatoes and potatoes—among the plants grown from the seeds of the graft. Professor Daniel mentions many examples of such hybrids in his writings about his work along this line. I have never experimented any in this direction, and hence cannot say how it would affect in combination with cross-pollination. I would expect less stability in such hybrids, if we may properly term them such, than those produced in cross-pollination.*

I have conjectured sometimes whether or not it is possible for double hybridization to take place in an ovule in a single operation by, at the same moment, having a spermatozoid from each of two or more species to enter the ovule egg-cell and fuse together with the protoplasm of the egg nucleus, thus combining the blood of three or more species all at once. As the blood of the several species can be eventually intermingled in one individual, by successive crossings, there would seem no physiological reason why not all at once, by applying mixed pollen to the stigma. But so far as the most painstaking microscopic scrutiny among the few species of plants examined reveals the process of fecundation by pollination, only one male nucleus has ever been found to reach and fuse with the female nucleus, although several spermatozoids have been observed lodged against the egg-cell wall of the ovule, and a number of pollen tubes containing spermatids have been seen at the same time bending into the archegonal chamber in the apex of the prothallus of the ovule.† It seems probable that the male and female nuclei are minute polarized protoplasmic masses. The germinal nuclei may be drawn to each other in a similar way that magnets attract each other.‡

The first spermatozoid to bore through the egg-cell wall by aid of its spiral ciliary band is at once focused in the magnetic pole of the female nucleus, which is much larger than the male nucleus, and drawn into it and fused together. The female nucleus seems incapable of focalizing on or attracting more than one male nucleus. They (the male and female nuclei) appear to have passed the stage of spermatozoids instantly at moment of

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*See also Mr. A. Jurie, in Revue des Hybrides Franco-Americains ou l'Isabelle de Poligny Hybride de Greffe.

†H. J. Webber, in Bulletin No. 2, Bureau of Plant Industry, Dep. Agr. This treats of fecundation in Zamia, but by the laws of comparative physiology we infer that the process is similar in Vitis, in which the process has never been observed. The flowers are too small, probably, even for the microscopic eye to observe it.

‡The Blepheroplasts, or specialized centrosomes, similarly to the rays of a magnet, curving back around and toward the central line of the central cell of the pollen tube, strongly suggest magnetic force, or a very near relative, controlling the life movements of the spermatid in the central cell, causing it to divide. But the formation of the ciliary band and the swimming in the cytoplasm by means of the cilia, and the subsequent boring of the spermatozoid into the egg-cell, by means if its gimlet-like cone, suggests more than mere blind magnetic force. It seems to be more akin to intelligent or conscious pairing.

contact, losing all affinity for other nuclei, and start at once a new life, the infant germ soon to become the mature seed. In some of my accidental hybrids, at times, when trying to analyze them by their specific markings, it would seem impossible to explain them as resulting from only two of any known species, some of them apparently embodying three and four distinct sets of specific markings, while their known mothers were apparently pure representatives of only one species, which stood surrounded by numerous other species, several flowering at the same time. Yet as nearly all our species of grapes show that they contain traces of mixture with other species at some distant past, reversion may explain these apparent complex hybrids by one generation. The theory of mutation, which appears to demand a causeless effect, I dare not appeal to until better proven. I throw out these thoughts for some of our younger, more scientific hybridizers to work upon.

If it should be found that double or treble fecundation can occur with the female nucleus, and can be practiced by the hybridizer, he could accomplish in one lifetime what would take generations to do by single pollination. The rate of development would be high geometrical, instead of arithmetical, or a low geometrical speed, as by present methods.

THE LIMITS OF USEFULNESS IN HYBRIDIZATION AMONG GRAPES.

In considering the first part of our subject, the objects to be sought were briefly pointed out. In general, we may say, that when all those objects served by hybridization of pure species are reached, we should cease such direct intermingling of pure species and proceed to select and cross-pollenize among the varieties already produced, to constitute a separate family, until we could work it into more desirable new varieties, all of a sufficient homogeneity to give a fixedness of type, that will yield uniform results, as in types of stock breeding (although live stock breeding does not make a perfect comparison, on account of lack of species involved, unless we should include in it several species, as the musk-ox, the bison, the Indian and African cattle, etc., in the genus Bos).

By thus cross-pollinizing we eventually can build more wonderful families of grapes for every season and use than the Old World has ever known among its thousands of varieties.

There are some fundamental facts with reference to pure and mixed blood that, to keep in mind, will greatly aid the hybridizer in the proper conduct of his work of development.

The purer the specific blood, known by the greater uniformity of the individuals of the species, and especially season of flowering, character and ripening of fruit, the more persistent to its type will it be in cultivation and in showing forth in hybrid forms. The hybrids vary much less from the parent taken from the homogeneous species than they do from the parent from the variable species. This law, or method of action, is well illustrated in quite a number of hybrids of *V. rotundifolia* (one of the most homogeneous species), with less homogeneous species and their hybrids, produced by me. Several French hybridizers, among them A. Millardet* of the Faculty of Bor-

*Deceased since this paper was read, in 1903.

deaux University, who is one of the most scientific and noted living grape hybridizers, have noted this law in their writings. Persistency or non-variableness of character in varieties under differing conditions of soil, climate, etc., is very desirable, and it gets it undoubtedly from purity of specific blood, or long continued selection and combinations of those varieties that best resist the maladies contended against. Hence, on this account, it is better not to combine more species in one variety than is necessary to include the points sought.

On the other hand, the greater the number of species, especially species of great variability within themselves, such as *Vinifera, Labrusca, Lincecumii, Æstivalis, Bicolor,* embodied in a hybrid, the greater will be its unstableness of character, tried under different conditions, so that each season being somewhat different from every other, one can never beforehand be certain of what the variety will do. Still more will it vary in character in being put into different soils and climates, and hence is certain to be very local in its successes.

To illustrate: Suppose that I should reason, like the quack doctor, that if I combine a great many drugs into a "pill" or "bitters" that it (the pill) would cure every disease, according to the theory that where one medicine would miss, another in the pill would hit, and conclude that if I should unite in one very complex hybrid all native American grapes that the complex hybrid produced would be a universal success; entirely overlooking the fact that the blood of extreme southern kinds would reduce the hardiness of the northern species against cold, and vice versa; that the species from the high, arid regions, where cryptogamic diseases cannot exist, intermingled with those in the lowland humid atmosphere would make hybrids that could neither resist much drouth nor the fungus diseases. So, the hybrid embodying all species, instead of being universally successful, would universally be a failure, just as is the quack's stomach bitters.

The true principle is to produce special varieties for special soils, climates and uses. But in every case we want those resistant, as far as possible, to insect and fungus diseases. We want larger clusters and persistent, larger berries for market and table. We want those for wine that will produce abundance of must of special desirable qualities. How can such results be best secured for each region? Clearly, only by combining two to four species, if so many can be found, of the very best selections of each in the region to be supplied. This gives room for each hybridizer to create a special list of varieties for the peculiar climatic region in which he works.

Here it may be acceptable to introduce some suggestive propositions.

Suppose I should desire to develop a set of varieties best adapted to the hot, dry climate of Southwestern Texas, to grow in the very limy upland soils there, where the Phylloxera is natively abundant. Would I not select the large clustered, fine quality, Phylloxera-leaf-folder-rot-resisting, deep-rooting, lime-loving *Berlandieri*, and intermingle it with the best disease-resisting, drouth-resisting, lime-loving, large-clustered, large-berried *Viniferas?* Most certainly I would, as I have already done in a small way, with most promising results, for a widely extended region in the Southwest. To give still

greater root power, with nearly all of the good points of the *Berlandieri,* with larger berries, highly colored juice, easy growth from cuttings and desirable flavors and wine properties, not found in either *Berlandieri* or *Vinifera,* native of the same region with *Berlandieri,* I would also surely use *V. Champini, V. rupestris,* and perhaps *V. candicans* for special wine purposes. If, on account of the non-resistance of *Vinifera* to Phylloxera, the hybrids might not be sufficiently resistant thereto, I would graft them upon *Berlandieri, Champini, Candicans* and *Rupestris* of that region, and continue to introduce into them the finer hybrid strains of native blood.

So I would do for other regions with their native species in combinations with the best selections of *Vinifera* for such regions. The *Vinifera* we cannot afford to omit, as it is the embodiment, in fine qualities, of thousands of years of improvement. We can fortify its weakness with native blood, and besides greatly enrich its qualities for every use. Yet we have produced hybrids without any *Vinifera* blood of very good qualities for market, table and wine, and could, so I confidently predict, erect a splendid viticulture, if *Vinifera* were all destroyed.

It is quite clear, from all our experience, that little can be gained by combining more than three or four species for any particular region, and those species should be, as far as possible, the best selected natives of the region to be supplied. This applies only to those parts of the United States east of the Rocky Mountains, for no other regions of the earth have any native species, except the *Vinifera,* suitable to yield any very good results in hybridization.

For other parts of the world some combination of disease-resisting American varieties with *Vinifera* promise the best results, as in France many direct producers, especially for wine, have been originated by combining *V. rupestris* chiefly, and *V. Lincecumii* with *Vinifera* The French taste greatly dislikes the "foxy" and earthy flavors of the *Labrusca* and its low percentage of sugar, hence eschews it for new-variety-making purposes.

The primary specific selections should always be, as nearly as possible, from similar climatic and soil conditions to those to be served.

If we should start with only one variety, or original selection, from each of two or three species of any particular region, and continue indefinitely to use these through a long course of selection and recrossing, eventually we would tend too much to in and in breeding to permanently maintain endurance. Such seems to have been somewhat the character of development of many *Vinifera* varieties, while others appear to have come down purely of one species by a long course of pure selection; and I believe this too close in and in breeding has had much to do with the feebleness of many varieties of that class of grapes. Hence, for each distinct set of climatic and soil conditions we should have recourse to several different varieties of each species used, collected from widely separated localities of that region, if possible.

For example: If we were endeavoring to produce the most desirable of market and table grapes for New England, the Atlantic Slope and the Great Lakes region, where the flavor of *Labrusca* is relished, we would begin by selecting Concord, Ives, Perkins, Wyoming, Columbian and others of the best

types of *Labrusca* yet found, along with Clinton (an accidental wild hybrid of *Labrusca* × *Vulpina*, found in New York) and the best of the Taylor family (accidentally originating in Kentucky as a hybrid of *Labrusca* × *Vulpina*), and we would include some of the very best of the *V. bicolor* of Ohio (such as Kohr and others found by Dr. G. L. Tinker of New Philadelphia, Ohio, who is most intelligently working to produce a family of *Bicolor* hybrids), and some of the large-berried, large-clustered *V. Lincecumii* of Southwest Missouri, Arkansas, the Indian Territory and North Texas (such as Jaeger's 43 and my Lucky, which have proven perfectly hardy in Massachusetts, Ohio and New York, and very resistant to rot and mildew), and intermingle them by twos and threes with each other and the hardiest, healthiest, large-clustered, large-berried *Vinifera* varieties.

Thus, several distinct families should be started, and from time to time various selections from these different families of hybrids should be chosen, according to their peculiar fitness for some special purpose or location, and combined to form secondary families, that would be decided improvements upon their progenitors. Such a large basis to use for development would serve for many generations, and the limits of evolution of colors, seasons, flavors, etc., could never be reached. Clusters requiring two men with a staff to carry them, as in old Canaan, might be produced, with berries on them as large as Kelsey plums! Who knows? If nature, by its haphazard selection, aided by the wild animals, produced the cocoanut, the banana, the bread fruit and the marvelous durion fruit of the tropics, what may not nature and man together produce, with scientific knowledge, keenly sharpened observation and skilled technique on the part of man?

As an example of cluster lengthening in two or three generations from varieties, none of which had half so long clusters, those of the Captain grape have come, and, withal, much better qualities. As an example of refining and compounding flavors by complex hybridization, I mention but one among a large number produced, of which this is an exact natural size cluster, reproduced somewhat nearly in color. It is named Wapanuka, and contains in combination at least three species, and probably four, partly by accidental and partly by careful hand hybridization.

This variety was produced by me by pollenizing my Rommel with Brilliant pollen. Rommel was produced by me by pollenizing Elvira with Campbell's Triumph. Elvira was produced by Jacob Rommel of Missouri from seed of Taylor, probably accidentally crossed by some *Labrusca*, unknown, or may have been a pure seedling of Taylor. Taylor was an accidental hybrid of some unknown *Labrusca* with some unknown *Vulpina*. Its blood is proven by its characters being of these two species, without doubt. Triumph was produced by the lamented George W. Campbell by pollenizing Concord with Muscat of Alexandria (*Vinifera*). Concord, as you all know, is a seedling of second generation by E. W. Bull from some wild *Labrusca* ("Fox Grape"), found in the woods by some boys near where Mr. Bull lived in Massachusetts, and given to him to taste. Brilliant was produced by me by pollenizing Roger's No. 9 (Lindley) with Delaware. Lindley was produced by E. S. Rogers, the pioneer hybridizer of grapes in America, by pollenizing some large *Labrusca* variety, found by him in the woods of Massachusetts, with

the Golden Chasselas. Delaware, found in a Mr. Provost's garden in New Jersey, clearly a hybrid between some *Labrusca* and either *V. Bourquiniana*, of the Herbemont type, or some *Vinifera* variety (Botanical markings are much more like *Bourquiniana* than *Vinifera*) ; or, speaking specifically, by proportion of parts, Wapanuka consists of the best strains of three or four species, namely: *Labrusca*, 8 or 9-16; *Vulpina*, 1 or 2-16; *Vinifera*, 4-16; *Bourquiniana*, 2-16; It is slightly subject to mildew—less than Delaware—which it inherits from the Delaware and the *Vinifera*, through the Lindley and Triumph. It has the delicate skin of *Vulpina* and *Vinifera*, thus proving that weakness survives in a variety through many generations; hence, the great importance of starting with varieties devoid of weakness, as well as with those possessing as many large merits as possible. While the Wapanuka has some weaknesses, it has the most delicate flavors and texture, making it exceedingly palatable, even surpassing the best *Viniferas*, although much more than one-half wild American blood, and the vine endures the climate well, both in Texas and Ohio, without protection.

I am quite sure that selection and hybridization conducted on the plans I have outlined will make the best stable progress in producing all manner of varieties desirable for the table, the market and for wine, for every region where any two or more species of grapes will grow, and that has proven to be more or less of the surface of every country of any considerable size, lying between north and south latitude of fifty degrees.

The Chair: I am quite sure we all feel indebted to Mr. Munson for his paper. It comes from one who has followed the work in this line for a long time, and the results of his labors are well known.

The following paper was read by C. E. Saunders,

NOTES ON SOME VARIATIONS IN THE SECOND GENERATION OF BERBERIS HYBRIDS

Chas. E. Saunders, Experimental Farm, Ottawa, Canada.

In May, 1894, at the suggestion of Dr. William Saunders, Director of the Experimental Farms of the Dominion of Canada, the writer of this paper crossed *Berberis Thunbergii* with *B. vulgaris purpurea,* the former being used as the female. Five flowers were operated upon and five seeds obtained. These were sown the following autumn, and from them four strong shrubs were raised, all of which are still living. They are practically identical in all respects, and need not, therefore, be discussed individually.

The principal characteristics of these hybrids have already been described in a paper read by Dr. William Saunders before the British Association for the Advancement of Science in 1897; and it is therefore unnecessary, at this time, to refer to them at any length. It may be well, however, to present in tabular form some of the chief points of difference between the hybrids and their parents:

	Habit of shrub	Size of leaves	Color of leaves	Spines on edges of leaves	Blossoms	Color and lustre of ripe berries
B. Thunbergii	Rather low and Spreading	Small	Green	None	Borne singly	Scarlet, glossy
B. vulgaris purpurea	Somewhat upright	Medium	Dark purple	Numerous	In bunches of 17 to 21	Dark red, dull
Hybrids	Intermediate	Under medium	Green	Very few	In bunches of 5 to 10	Scarlet, dull

It will be seen from this summary that in almost every respect the hybrids stand midway between the parents, neither the male nor the female seeming to have exerted a preponderating influence on the progeny.

As the hybrids all produce seed in considerable quantity there has been no difficulty in raising a second generation of shrubs to the number of about a thousand. These are now growing on the Experimental Farm at Ottawa.

Uniformity is the striking characteristic of the first generation; variation is the predominant feature of the second. While many of the seedlings are not yet old enough to bear blossoms and fruit, they have already shown

numerous interesting departures from the uniform type of the first generation, the characteristics of *B. Thunbergii* coming out very strongly in some instances, and those of *B. vulgaris purpurea* coming out equally strongly in others.

The leaves of these seedlings in the second generation vary in size very much, being in some cases even smaller than those of *B. Thunbergii,* and in others even larger than those of *B. vulgaris purpurea,* the habit of the shrub frequently following that of the species which its leaves more nearly resemble. The margins of the leaves are in some instances free from spines, yet they are in some other cases provided with a larger number of spines than are found on the leaves of the parent hybrids.

The color of the foliage is, however, perhaps the point of chief interest; for, while in the hybrids of the first generation scarcely any tendency to purple can be detected, the second generation gives numerous shrubs of as deep a purple as *B. vulgaris purpurea* (or, perhaps, even deeper), some of them having also large leaves and a vigorous, upright habit. The shrubs with purple foliage constitute about twenty-three per cent. of the whole number grown.

The reappearance of this "recessive" character in almost exactly one-fourth of the seedlings is of interest in connection with Mendel's observations on cross-bred peas.

The Chair: I have listened with a great deal of interest to this hybridization that Dr. Saunders has just spoken of. It stimulates the hope that there may be practical results of importance. For to my taste there is no jelly from whatever fruit that equals that made from the barberry, and if you can get something that will be productive of better results in this useful particular I am sure great gain will have been made, beside the incidental scientific interest of the work as it proceeds.

The following paper was read by R. M. Kellogg.

BUD VARIATION IN THE STRAWBERRY PLANT

R. M. Kellogg, Three Rivers, Mich.

No plant affords better opportunities for studying bud variations than the strawberry. New plants can be propagated and fruited every year, and while to the novice they all appear alike, yet a careful examination will show constant changes going on in its vascular system making it possible to greatly improve them through the agency of selection.

Bud variation may be defined as any change in the vascular system of a plant which shall cause it to produce a different fruit either in form, quality or quantity.

To me it is surpassingly strange that the theory has been so generally accepted that all plants and trees propagated by buds are stable in their character, when the orchardist can find, not only certain trees but many limbs on bearing trees which produce different fruit for years in succession, and the berry grower can hardly walk through his plant bed without seeing a variety of types in fruit and a vastly different degree in their productiveness. The complexity in the organism of the plant is as great as that of the man, and shall we say that a man born weak in parts of the body shall spend his days without change? Is not the college curriculum for the development of the brain and the gymnasium for the weak parts of the body? Was the athlete born an athlete or is he a product of development after his birth. Here we have no argument, but when it comes to developing any part of the physical organism of a plant so as to make it produce different results, it is said that it cannot be done. This is an error which has misled the fruit grower and robbed him of the pleasures and profits of his business.

For the past nineteen years I have conducted experimental plats with the view of devising means of developing a stronger fruit-producing organism in the strawberry plant while in the nursery bed, and results of these experiments have proven conclusively that it can be done.

From the first I have felt the force of the remark by Prof. Bailey that "We need not so much varieties with new names as we do a general increase in productiveness and efficiency of the types we already possess," and so my efforts have been directed to breeding into the plant a stronger vascular fruit-producing organism by favorable environment and continual selection. To determine if such change could be effected, I made experiments along as widely divergent lines as possible, holding that if there was

no variation except as produced by manure and tillage and that of a temporary nature, we could subject one set of plants to severe strains and hardship and when restored to congenial environment they would produce fruit of as even type as the one continued under favorable conditions.

Plants were selected of three varieties and grown on the same soil through a period of six years under the following conditions:

Plat No. 1 was manured heavily with nitrogen, the plants set close together and allowed to fruit the first year and mat very thickly. They were given no winter covering and the following spring were allowed to fruit all they could, and continued in fruit for four years, narrowing the bed each year and allowing it to mat thick again under the influence of the stimulus of nitrogen.

Plat No. 2 having the same varieties were put in land rich in potash and phosphoric acid and very moderately so in nitrogen and grown in hills to preserve their individuality and enable me to more perfectly observe variations. The first spring they were given extra care in all respects. The blossom buds were all removed, the runners cut as fast as they appeared and fully protected with mulch during the winter. The following spring a careful examination was made for variations. Those having the most perfect crowns and general vigor were scaled and blossom buds except four on each stem removed to prevent excessive pollenation and when fruit had set, two berries only were allowed to ripen. The one scaling the highest and showing the most perfect fruit was selected for propagating for plants the next spring. The selected plant was watered and induced to throw out runners, which were potted and transferred to a special bed, where they were allowed to make runners for next spring's planting, the same process being repeated each year.

At the end of four years plants were selected from each plat having as nearly the same rootage as possible and each variety set in alternate rows and all grown in hills under the most thorough tillage and carefully mulched during the winter.

It is worthy of note that plants from Plat No. 1 made several times as many runners, and when cut they persisted in throwing out others, and at the harvest time there was a very wide difference in fruitage in the individual plants both in quantity and type.

From Plat No. 2 we found that when runners were cut, new crowns readily formed, and the fruitage of each plant was generally even and close to type, and this difference was very marked the following year. In the first case I had developed the vegetative organism of the plant so that its physical ability was directed to producing runners and foliage that I did not want, and in the other I had created a strong fruit producing organism and a foliage strong enough to support it in assimilating all the food required by the fruit-producing vascular bundles, and by this system of selection we used only the strongest plants, while weaklings and bad variations were annually thrown out.

The general average of production among fruit growers is from fifty to seventy-five bushels per acre of inferior berries, and this very low yield in quantity and quality is due to the manner of propagating their plants.

Strawberry growers very generally practice what is known as the wide matted row, the plants being thrown around on the edge of the row by the cultivator so that sunshine is shut out from the crowns, and for renewing their beds it is a universal custom to take the plants which form in the path or alley late in September and October, giving them no adequate time to perfect their buds, and thus the vegetative part of the plant was stimulated and the fruit-producing organism repressed so that heavy manuring and tillage resulted in producing excessive runners and foliage without a corresponding increase in fruit.

For my fruiting fields I have for years grown all my plants from ideal or perfect specimens found here and there in the field, beginning the search for them in the growing season, and those most promising—showing large fruit crowns and healthy foliage—were staked and numbered and the following spring restricted by removing half the blossom buds to prevent pollen exhaustion. After fruit had set, only two berries on each fruit stem were allowed to ripen so that the form, texture, flavor and color of the berries might be determined. Each plant was scaled on the basis of one to ten, and the one showing the greatest number of points of excellence was given the "blue ribbon" and became the mother of all the future plants of that variety on the farm.

The runners were potted and transferred to special beds, where they were given room for plenty of air and sunshine to further develop and stimulate their fruit-producing organism, and the following spring all new beds were stocked with these plants.

To determine if results justified this process of plant-growing, I set plants from a neighboring plantation grown after the general plan of fruit growers, taking "alley plants" from the edges of the matted row, setting them in alternate rows with those thoroughbred as above stated, giving all the most thorough tillage and confining them to hills and hedge-row. There was a wide difference in the quality and quantity of the berries picked from the two classes of plants. As in the case of the first experiments, the selected plants gave at the rate of from three to five hundred bushels per acre, while from the plants of the second grade scarcely a hundred bushels were secured.

It has been estimated that the strawberry production in the United States has now reached the enormous sum of ninety million dollars in reasonably favorable years. The demand is governed by quality or the pleasure experienced in eating the fruit, and so if growers can be induced to adopt the better methods of breeding their plants and giving them the benefit of modern research in tillage and fertilizing, the question of markets would be solved by greatly increased consumption creating a demand that could hardly be met at largely advanced prices.

The berry grower is now just where the stock grower was fifty years ago. Then stockmen talked only of breeds. If the pedigree was right, any sway-backed, knock-kneed, badly developed animal would serve the full purpose for breeding, but it is worthy of note that the splendid animals which now grace our barnyards did not come into existence until the individuality of an animal fixed its value.

At all our horticultural fairs and meetings we talk incessantly of varieties without regard to plant individuality. We never put stress on the physical condition of plants under test but class them all on an equality. New varieties come and shine like a meteor in the horticultural heavens for a season, and then through a want of restriction and selection of better variations, they produce fewer berries of lower quality, and finally drop out of sight while the new seedlings follow in rapid succession.

In the not distant future our agricultural colleges will give us experts who will detect these valuable variations, and our nurseries will furnish us plants and trees handed down through generations as the accumulations of better qualities through constant selection of bud variation just as they are now doing in breeding corn, cotton and wheat. We must no longer breed our plants at random, but do it with a definite object in view.

Let us have hybridization to secure initial changes and then, with the mind centered on the ideal, seek the slower process of bud variation as a means of developing and fixing in the plant the desired changes, and then we shall see the dawn of a new era in horticulture.

H. F. Roberts: Have you made any crosses on any of these improved bud variations?

R. M. Kellogg: No, sir; I have felt that this is an age of specialties. I used to dabble in almost everything, but the last three years I have found all my time fully occupied in this one point. Take the popular varieties to-day handled by nurserymen, and you will find that they all center on less than twelve varieties that are leading popular sorts. This has arisen out of the fact that we cannot get a superior variety on an average of less than 20,000 seedlings, and it is true that these are required to be developed by a system of thorough tillage through many years, and that is the cause of the plants running out. Strawberry growers never prune their plants. They never restrict them. They bear all the pollen they can, and seed bearing takes place, and it runs the plant out so it is physically unable to produce seed, and as the fruit flesh grows only as the substance for the seeds to grow in, you soon run them out. Now, this has been my work: Simply building up in the plant the seed organism and consequent fruit development, and, therefore, I have not paid any attention to new selections, but have taken those produced or found by other people and stimulated them by thorough tillage, plenty of air and under the most favorable conditions, using largely phosphates and potash—as you know, those stimulate the seed-bearing organism—and bring out the plants in that way. The statement has been made, I don't vouch for that, that ninety millions of dollars are spent every year in strawberries, and yet I believe that one-half of that crop is lost, simply because of plants that by devitalization have been simply rendered incapable of bearing fruit. You prune your orchards; you prune everything else; why shouldn't you prune your strawberries?

The Chair: We will have one more paper at this session that will not take quite all of our time, and so I ask Mr. G. T. Powell to speak to us upon some of the results obtained by him in bud variation in apples.

BUD VARIATION IN THE APPLE

Geo. T. Powell, Ghent, N. Y.

Ten years ago I began the propagation of two kinds of apples, the Tompkins County King and the Sutton Beauty, upon the principle of bud selection. While at Geneva I observed on the grounds of Mr. S. D. Willard a Sutton Beauty apple tree, one portion of which showed decided characteristics in quality of fruit better than other portions of the tree. I selected from this portion of the tree scions, and top-worked 100 Northern Spy trees with these grafts from this Sutton Beauty apple tree of Mr. Willard's. I have since selected from the trees thus propagated the strongest, finest buds from the most typical trees of the 100 so started originally.

I have to-day the current generation, that is, the result of the third selection of the Sutton Beauty, each time selecting the finest type of tree, then studying the character of the growth of this tree and choosing the buds from the strongest, the most vigorous branches upon these trees.

Now, there were three points in my mind in starting this work. One was to get vigor in the growth of the tree. The second was to get uniformity in the character of the fruit, and the third was to obtain, if possible, the prolific tendency. So far I am very glad to report that all three points have been well secured, as this third result of the fruit which I have got.

In relation to the Tompkins County King another very interesting result has been observed; that is, by top-working the Tompkins County King buds upon the Northern Spy tree, the constitutional weakness of the King seems to be strengthened; that is, it seems to be eliminated so far, because, after eleven years, there is not the first evidence of apple canker appearing upon these young Kings top-worked upon the Northern Spy stock, while ordinarily in New York State at the age of eleven years there are from 15 to 30 per cent. of the trees so affected with the apple canker that they become practically valueless. I believe that there is a wonderful field in this direction; that there is as much difference in the buds upon the tree as Mr. Munson stated that there is in animals in breeding. I am very glad to give just these few words in these very few moments.

The following paper was read by H. C. Price.

HAND POLLINATION OF ORCHARD FRUITS

H. C. Price, Horticultural State College, Ames, Iowa.

To the plant breeder thoroughly endowed with a love of his work and appreciating the possibilities that lie before him, the Northwest offers an enchanting field for labor. The prairie conditions of the Mississippi Valley are peculiar to themselves in soil and climate. No place on the globe has an equal body of land abounding in such agricultural wealth. Tempered by no inland lakes, sheltered by no surrounding mountains, and protected by no native forests, our conditions are extremely severe.

The first settlers brought with them the varieties of fruit they had grown in the East, and saw them "go out," unable to stand the trying conditions. The country was scoured for hardier varieties, but with little success. Importations were made from other countries, notably Eastern Europe and Western Asia, and at one time it was thought that a panacea had been found for all troubles in this foreign stock. They were heralded as cold resisting, disease resisting and insect resisting, but experience has taught that the real value of a large majority of them will be as a means to the end in the hands of the plant breeder rather than in any inherent value they may possess themselves.

The plant breeder has the opportunity now to gather up the broken threads of these dismal failures, and by bringing together the good qualities and eliminating the bad to produce the longed for fruits.

To do this we must awaken an interest in plant breeding among horticulturists; we must have their co-operation. The task that to one man may seem hopeless, to a thousand is but recreation. For one man to hand-pollinate one hundred blossoms each spring is not much of a task, but if 1,000 men would do this we would have 100,000 blossoms bred each spring.

The production of hardy varieties is the work for the masses, and not for the single experimenter. Success in plant breeding is best obtained by raising large numbers of seedlings, and destroying with a free hand. Hand-pollination is too tedious work for one man to do a great deal of in the short time that he can work. The operation itself is not difficult, and can be done by any one with a little practice.

In order to get horticulturists to co-operate in this work the Iowa Experiment Station has been sending pollen to any that would agree to do the work under their directions. Pollen is gathered in the southern part of the State, and is distributed from the Experiment Station. Certain crosses are recommended, and the pollen is supplied to make these crosses. In the fall the

seeds of the hand-pollinated fruit can either be sent to the Experiment Station to be grown on their grounds, or kept and grown by the man raising them, and the latter method is preferred. It keeps the man's interest alive in the work to have his own seedlings growing before him, and for him to watch for their first fruits. He not only watches them himself, but he shows them to his friends and awakens their interest, and thus it keeps spreading from one farm to another, and the interests of plant breeding are advanced.

Horticulturists often hesitate to take up the work of hand-pollination because of the tediousness of the work. At best it is a slow and painstaking operation. It comes at a season when the horticulturist is crowded with work, and the short time that the blooming period lasts necessitates that all the work must be crowded into a very few days. Anything that will facilitate the details of the operation is of prime importance to the plant breeder. In order to determine some way of lessening the details of the work, the Iowa Experiment Station last spring made comparative tests of different methods of pollinating the apple.

HIGH VERSUS LOW EMASCULATION.

In high emasculation the corolla and anthers were removed with tweezers, and the calyx was not disturbed. In low emasculation the calyx, corolla and stamens were removed by cutting through the blossom just at the base of the sepals, being careful not to injure the pistils. This necessitates very careful work and cutting entirely around the blossom. Low emasculation has the advantage that it can be done much more expeditiously than high emasculation, and if the results of pollination were satisfactory would be a decided advantage. But in fifty-nine blossoms emasculated low in five different varieties of apples only seven fruits set, or less than 2 per cent.; and in eighty-eight blossoms emasculated high twenty-three set fruit, or slightly over 26 per cent. The results were even more pronounced in the Wealthy apple. Fifty blossoms emasculated high and pollinated with Ben Davis pollen set eleven fruits, or 22 per cent. Twenty-five emasculated low set only one fruit, or 4 per cent.

POLLINATION AT TIME OF EMASCULATION.

It is a commonly accepted idea that better results are obtained from pollination if delayed for two or three days after emasculation, or until after the stigmas come into a receptive condition, as shown by the viscid fluid that shows on their surface. To follow this method necessitates the covering of the blossoms when emasculated and then going over them again to pollinate them, and resacking. A test was made of the effect of pollinating as soon as emasculated, and of delaying it two or three days till the stigmas should come into a receptive condition. The results were very decidedly in favor of the immediate pollination. Out of 134 blossoms, consisting of Wealthy, Walbridge and Grant Crab, mostly Wealthy, pollinated as soon as emasculated, 95 fruit set, or a fraction less than 71 per cent., while in 83 blossoms of the same varieties, pollinated three and four days later (the weather being cool), only 13 fruit set, or a trifle over 15 per cent.

The blossoms were emasculated in the ordinary method, removing only corolla and anthers. These results would seem to indicate that better results can be obtained if the pollen is placed on the stigmas as soon as the blossom

is emasculated. When put on in this manner it awaits until the stigma comes into the receptive stage and then germinates and grows down and fecundates the ovules. On the other hand, if delayed till the stigma shows to the eye that it is in a receptive condition the style is likely to break down before the pollen reaches the ovules. Or, in short, it is much safer to put the pollen on the stigma too soon than too late.

APPLYING POLLEN TO THE STIGMA.

The question of how the pollen shall be applied to the stigma is one of the important details of the work. The use of a camel's hair brush is usually recommended, while others say that as good results and more rapid work can be done by using the finger. In our experiments, out of 73 blossoms of Wealthy and Grant Crab apples pollinated with the operator's finger, 12 fruit set, or 16½ per cent., and out of 53 blossoms of the same varieties pollinated with a camel's hair brush, 14 fruit set, or 26½ per cent. If the supply of pollen is abundant, it can probably be supplied as satisfactorily with the finger as with the brush, but where the supply is limited and must be used sparingly it is likely that more satisfactory results can be obtained by the use of a brush.

The most pronounced results obtained were those that favor pollination at the time of emasculation. Also, in collecting pollen, it was found that much better pollen was secured from anthers taken from blossoms before the corolla had opened. In general I believe that more satisfactory results can be obtained from hand-pollination by doing the work earlier than is generally practised.

Hand-pollination is the highest developed art of the plant breeder. By it he is enabled to control his conditions and establish pedigrees with as much certainty as the animal breeder. Inter-planting, natural crossing and various methods of securing cross-pollination may be practised, but they can never establish positive records that will enable the scientist to establish the laws of plant breeding. The scientific plant breeder must continue to follow hand-pollination, and in orchard fruits in which the generations are so slow in reproducing themselves, it is doubly important.

The conference then adjourned to the Museum of the New York Botanical Garden.

The following paper was read by H. F. Roberts:

METHODS OF CEREAL BREEDING IN KANSAS

By H. F. Roberts, Botanist at the State College, Manhattan, Kansas.

The botanical and chemical departments of the Kansas State Agricultural College, at Manhattan, Kansas, have in charge, conjointly, the work in plant breeding at the Kansas Experiment Station. Hitherto our efforts have been concentrated upon the improvement of wheat and corn. During the present season, however, the breeding experiments have been extended to cover rye, oats, barley, kafir corn, soy beans and cow peas. But since the investigations have thus far been confined exclusively to wheat and corn, it is to a discussion of our work with these cereals that this paper will chiefly confine itself.

Whether the cereal wheat has arisen from a few or from many wild forms, it is certainly true that the wheat of to-day consists of a very great number of sub-varieties. A visit to almost any average wheat field reveals the presence of several such varieties, which, despite the various manifest differences in the character and quality of their seed, in the quantity of yield, in the vegetative characters of the plants, etc., are allowed to grow undisturbed together, in the end are harvested together, and the heterogeneous mixture of seeds is used again as a basis for the next year's sowing. Now and then one finds an occasional wheatgrower who endeavors by careful selection to breed his wheat up to a uniform type. Such instances, however, are not numerous, nor is it even the case that a large number of wheat raisers practice even the simplest form of seed selection. Indeed, I have found it to be the case in many instances, that farmers will systematically retain only the lightest and least serviceable seed for planting, under the impression that it will yield just as well as the larger, plumper and heavier seed which they dispose of in the market.

Our experiments in the breeding of wheat began in the fall of 1898. Since that time we have originated in the neighborhood of 150 crossbred strains of winter wheat. Of these, 36 are now growing in our experimental plots at Manhattan, the remainder having been gradually eliminated through a rigid process of selection, and through extinction in the face of trying climatic conditions. As the result of these crosses we have secured enough varying lines to enable us to begin the process of breeding by selection.

That portion of the State of Kansas in which the experiment station is located lies in what is known as the red winter wheat region, and our problems in wheat breeding have largely been connected with the improvement of wheat locally grown and especially adapted to that district. The western

third of the State, however, lying in a region of greater elevation, with a smaller annual precipitation and more rapid evaporation, presents conditions, during at least part of the year, which are frequently very unfavorable for the growing of wheat of the mesophytic type. Since it has been found, however, that the climatic conditions there are almost a counterpart of those in Southeastern Europe, and notably in Southern Russia, where the macaroni wheats are grown, a new possibility presents itself with regard to the breeding of xerophytic bread wheats for the elevated region of the great plains. Inasmuch as the macaroni wheats are not adapted to breadmaking, it will probably always be the tendency of the majority of our Western growers to raise bread wheat where they can and macaroni wheat where they must. We have, therefore, proceeded to make a number of crosses between macaroni wheats and some of our hardier winter bread wheats. We are also endeavoring to increase the drought-resistant qualities of our local bread wheat by crossing with spelts and emmers. The recent acquisition by the State of Kansas from the Federal Government of 3,800 acres of land in the semi-arid portion of the State and its transfer to the Agricultural College gives us a favorable field of operation for the working out of this particular problem.

Since, as I have stated, hard red winter wheat is the favorite milling sort with us, it is to those strains passing under the general name of "Turkey" that we have naturally turned for a basis, both for our work in cross breeding and in selection. For the red winter wheat district the Turkey wheat, passing under the name of red winter and obtained a few years ago from the Iowa Experiment Station, has proved in the course of several trying seasons to be in every way the hardiest and most drought-resistant sort. Our attention is now turned, therefore, toward the improvement of this wheat by a rigid process of selection and by cross breeding with other desirable varieties. In dealing with this variety and its crosses, the two most important questions with which we are concerned are, first, earliness, and, second, increased yield. We have been endeavoring to shorten its growing period by crossing with our earliest local variety, a soft, bald wheat, passing under the name of Zimmerman.

Naturally, the most pressing problem is that of increasing the yield of wheat, which is certainly far below what the fertility of the soil in Kansas would seem to justify. As was stated, there exists at present among our farmers, for the most part, no recognition of the advantages to be derived in the way of increased crop production from the selection of large, heavy seed. Quite recently, however, the seed-grading machines are beginning to find their way into our agricultural communities, and it is to be hoped that the use of these machines, by means of which the continued and persistent selection from year to year of large heavy grains of wheat for seeding is rendered possible on a commercial scale, will eventually result in increasing the product per acre considerably above the present standard. But it is well known to the plant breeder that the process of seed selection must be far more unremitting and far more rigid in method than the average wheat planter has the knowledge or patience to make it. It is fundamentally necessary that a strain of plants in order to be constant in their characters be botanically pure, that is to say, all of the plants must have descended from a common ancestor. Only in this way can a mixture of mutation forms in the sense of De Vries be avoided. No

process of selection in the case of wheat is scientifically accurate, therefore, or will yield permanent results which does not take account of the possible existence of these mutation forms and set about to discover and isolate them in pure cultures, which can then be compared with respect to all the desired characters sought for in wheat and only the most desirable and advantageous forms be retained.

Where selection is practiced at all by growers it usually consists simply in the choice of large well developed heads out of the mass of the ripened wheat plants. This method, of course, ignores the plant as an individual. It is necessary, therefore, after securing a pure strain, botanically speaking, to enter upon a very extensive process of selection of individual plants from among large numbers, grown in such a way as to enable each individual to attain to its maximum development. In this way what are known as the "tillering" qualities of the different individual plants, that is, their tendency to send up numbers of grain-bearing shoots from stolons, will be revealed. The collection at random of occasional large, well-developed heads ignores the possibility that such heads may be borne on plants markedly inferior in tillering capacity. As a basis for selection, therefore, we have this year planted our choicest varieties in nursery plots, in which the plants stand four inches apart each way. All of the seed thus planted has been carefully selected by hand, all but the largest and heaviest seeds being rejected.

An example of a search for an advantageous mutation form or sub-species, supposed to vary in the direction of increased flowering capacity, may be interesting. This year in the field of a supposed pure strain of Pedigree Early Genesee Giant (Kansas No. 147) close examination discovered seventy-two heads having a decided "club" tendency. It is a well-known fact that the extraordinary yields of wheat on the Pacific Coast are due in considerable measure to the fact that the wheats there grown are what are locally known as the "club" wheats, that is to say, wheats having the tendency to develop in the upper spikelets of the head five or more grains instead of the two or three usually found in our local winter wheats. This tendency results in a swollen or clubbed appearance of the head. The Pacific Coast club wheats, however, are not hardy varieties with us, and, therefore, are not available for introduction into our district. We must, therefore, look to the development of clubbed tendencies among our native winter wheats. In our two best hard and soft strains, Turkey and Zimmerman, this tendency does not appear at all. It does show itself in a number of our velvet chaff wheats and in the No. 147 just mentioned. Eleven of the heads of this number which showed the clubbed tendency most strikingly were selected, and a permanent record of their appearance preserved in photographs. From each one of these heads the spikelets were then carefully removed, from the base to the apex, in the order of their attachment to the axis. In each spikelet the number of flowers and the number of grains produced were tabulated, and are graphically indicated as illustrated in the accompanying diagrams. From these it will be seen that there is a marked tendency in certain heads toward the development of five and six flowers on the spikelet. In head No. 4, for example, spikelets 4, 5, 6. 7, 8, 9, 10, 11, 12 and 13—ten in all out of a total number of twenty-three spikelets growing on the head—produced six flowers. In seven cases five out of the six

flowers were found to have developed seed, making a percentage of fertility of eighty-three and one-third per cent. in one-third of the spikelets of the head. Moreover, out of the total of sixty-five grains borne on this particular head, it appears that thirty-five, or more than fifty per cent., were borne on these seven spikelets.

The attempt will be made to ascertain whether the use as parents for crossing, of plants coming from grains in the more prolific spikelets will be found to result in the production of offspring whose tendency will be to increase flower and seed production. It may be urged that the club tendency in this case was simply the result of great vegetative vigor and is not the evidence of a mutation form. On account of the severity of the season and the great lack of rainfall during the period when the heads were being formed on the shoots this seems unlikely.

To ascertain, however, whether we are coming into possession of mutation forms having increased flower production as a distinguishing character we are proceeding in the following manner: The grains in each one of the eleven heads mentioned have been planted this fall in separate rows. In each row the grains from the individual spikelets are arranged in their order from the base to the apex of the spikelet, and the grains of each spikelet succeed each other in serial order in the row. Each grain that grows, therefore, will be definitely referable to its place on the diagram. Next spring all the heads from each plant that grows will be harvested separately, and it is hoped that a statistical analysis of the progeny of the grains from all the different spikelets of these eleven heads will reveal at least something which may throw light on the facts we are seeking to discover. As a basis for selection of clubbed forms on a larger scale, in case this seems desirable, the remaining sixty-one of the clubbed heads of No. 147 have been planted in nursery plots, in which each head is planted by itself, but without any attempt to arrange serially the grains of the different spikelets.

As an example of the part which natural selection plays in the work of wheat breeding it may be mentioned that out of some three hundred plots of our native cross and pure bred wheats planted last fall ninety-nine were discarded in the spring on account of winter killing, while out of one hundred and eight imported wheats from Southeastern Europe furnished by the United States Department of Agriculture fifty-five were discarded for the same reason. Some of these turned out to be spring wheats and "durums," whose survival of the winter was not to be expected.

Further experiments are being carried on in the experiment station to determine the relative value of large and small seeds in cereal growing. Experiments conducted at the United States Department of Agriculture and elsewhere indicate a marked difference in plants growing from large as compared with those growing from small seeds. Experiments are being conducted by us to demonstrate this fact in the case of the cereal grains and to use the information obtained as a basis for urging the general use of seed-grading machines among our farmers.

As an economic cereal Indian corn ranks as an equal with wheat in Kansas in point of acreage. Thus far our experiments in corn breeding have been directed almost entirely toward the increasing of the nitrogen content. Since

1898 five hundred and eighty-one cross-bred varieties of corn have been produced at our station. Each one of these, beginning with the year 1899, has been analyzed by the chemical department to determine the protein content, and each year all those numbers showing a nitrogen content below two per cent. have been discarded. The results of the analyses for three years show seventy-nine varieties that exceed two per cent. in nitrogen, while twelve of these contain over two and four-tenths per cent. of nitrogen, which represents over fifteen per cent. of protein content. Of course, it will be understood that all the numbers thus analyzed have been hand-pollinated each year and protected from the influence of foreign pollen. It remains for further analyses to demonstrate whether this increased nitrogen content is a character which can be maintained under field conditions and over large areas.

Our efforts are further being directed, beginning with the present year, toward the improvement of corn according to a series of commercial standards such as those which have been laid down by the Illinois Corn Breeding Association and kindred organizations.

W. Saunders: Can Professor Roberts give me information in regard to a matter I was seeking information on? I lately obtained some wheats from Oregon, one under the name of Club and the other under the name of Blue Stem, and these wheats are said in Oregon to produce a good crop, whether sown in the autumn or sown in the spring. They can practically either be treated as spring wheats or winter wheats. That is, it the winter grain does not come through in good condition, all the farmer has to do is to sow a little in the bare places some of the grain in the spring, and it will ripen with the seed sown the fall before. Is it true or not?

H. F. Roberts: I don't know anything about that. That sounds like a California story.

W. J. Spillman: Mr. Chairman, I am from Oregon and Washington, and I can answer the question. I have grown wheat out there for ten years, and I want to say there are three varieties of spring wheat which are almost universally grown in that great region out there, two being Club wheats, the Blue Stem being a peculiar local variety of long-headed wheat not closely related to the Blue Stem of Minnesota, being another variety altogether. Those are all spring wheats; that is, if sown in the spring they will ripen and bear a crop. But they are universally sown in the fall of the year, because any wheat in that section, when it does not freeze out, will yield from twenty to fifty per cent. more if sown in the fall than in the spring; for that reason they sow them in the fall. But they have been searching for a better wheat out there, and I undertook the job of finding a winter wheat in that section. I first sent several hundred letters to farmers asking about suggestions as to what the experiment station should work for, and every man who answered the letter made the same suggestion; that was, "Give us a winter wheat that is adapted to this country. We sow the spring wheats in the fall, and they freeze out in the wintertime." The winter before last they had to sow them all again in the spring. And we went to work and experimented with several hundred varieties of winter wheat and found that we were chasing a wild goose. We went to work and began breeding wheats and got immediately what we wanted, and discovered Mendel's law in operation.

The Chair: I think it would be highly gratifying to Mendel if he could hear how his law is being demonstrated from both lines of approach, working forward and backward. We have an illustration of it here.

The following paper was read by W. Fawcett:

NOTES ON PLANT BREEDING IN JAMAICA

By William Fawcett, Director of Gardens and Plantations, Jamaica, B. W. I.

PINE APPLE.

The smooth Cayenne pine is the favorite in the markets, because it is an excellent fruit with a fine appearance.

There is no doubt, however, that the Ripley has a much better flavor, and many people in Jamaica will not eat any other variety.

We have begun, therefore, experimenting in crossing these two varieties, with the aim of getting a fruit with the flavor of the Ripley and the showy qualities of the Cayenne.

Prof. Herbert J. Webber, at the Conference at Chiswick in 1899, expressed the opinion that the flowers were self-sterile, and this opinion is confirmed by all the facts known.

In hot-houses no seed is produced, unless cross-fertilization is effected; but in the open fields in Jamaica, where humming birds can be seen probing the flowers, seed is frequently found and sometimes in great abundance.

The fact that the flowers are self-sterile simplifies the operation of cross-fertilization, as there is no need of emasculation.

We have now 51 plants grown from seed produced last year. Forty-three of these were from the Ripley, green and red mixed, fertilized with pollen from the smooth Cayenne.

Of these 43 plants, 33 have spiny leaves and 10 have smooth leaves. Some of the spiny plants have nevertheless green leaves much like the Cayenne, and some of the smooth-leaved plants have bright red leaves like the Red Ripley. All the intermediate variations exist, no two being alike.

Eight plants were the result of crossing the Porto Rico pine with the pollen of the Ripley. Two are like Porto Rico, 3 like Red Ripley, 1 like Green Ripley, and there are 2 smooth-leaved plants.

We have also about 2,000 seedlings from this year's plants, but they are still too young to show characteristic leaves.

BANANA.

If the flowering stalk is examined in the embryo condition in the stem, it will be found that the flowers are arranged in clusters disposed spirally round the axis. The clusters at the base of the stalk become the "hands" of the fruiting bunch. It will also be found that the flowers in different regions of the stalk vary in the proportion of the length of the ovary to that of the rest of the flower. In those clusters which eventually become

"hands," the ovary is two-thirds the length of the whole flower; higher up on the stalk are clusters in which the ovary is about one-half the length of the flower; and still higher there is another series in which the ovary is about one-third of the flower. These three sets of flowers, clearly distinguishable by the different proportionate length of the ovary, are physiologically very different; those with the long ovary are female flowers and become the fruit; those with the short ovary are male flowers, and those with the ovary about half the length of the flower are hermaphrodite and form short, useless fingers in the bunch.

The pollen in both the hermaphrodite and male flowers appears to be perfect.

We have tried to cross the red banana with the common Jamaican by applying pollen from male flowers of the red to the stigmas of the Jamaican.

Several seeds were obtained but they failed to germinate. They were about 6 millimeters long, and it is possible they were not perfect.

MANGO.

The Mango was introduced into Jamaica about 120 years ago.

The seed germinated so readily, even when thrown away along the road-sides, that it is now one of the commonest trees in Jamaica.

The fruit of most of these trees is stringy with a strong turpentine taste. It would be an advantage if they could be budded from trees bearing fruit of good quality.

It would also be advantageous if seedlings could be budded, as citrus plants are done. At present we propagate by grafting by approach.

Experiments so far in budding have not been very successful, only two buds having taken on young branches of trees. The sap seems to be so gummy that actual contact of scion and host is very difficult.

AVOCADO PEAR (PERSEA GRATISSIMA).

The fruit known as Avocado Pear is extensively used in the West Indies, eaten with pepper and salt with fish and meat. The best varieties have an excellent nutty flavor, but very many are insipid.

We have budded several plants at Hope Gardens, and demonstrated the practicability of improving the fruit generally in this way.

FINAL SESSION AND RESOLUTIONS

FINAL SESSION

(Held in the Museum Building of the New Botanical Garden.)

A session of the conference on October 2 was held in the museum of the New York Botanical Garden, by invitation of the board of managers. The members of the conference were entertained to lunch. President Wood called the conference to order at 2 p. m.

THE CHAIR: I desire to take this opportunity to express, on behalf of the Horticultural Society of New York and on behalf of this conference, the appreciation we have of the attendance at this conference of those gentlemen who have come to it from abroad, those who have come from across the water, the other side of the Atlantic, and those who have come to us from the West Indies, and also our brethren from Canada. We do not speak of the Canadians exactly as foreigners; while we recognize their attachment to their country and their intense loyalty to their sovereign, we always look upon them as Americans and as brothers with whom we grasp hands most lovingly. But the attendance of these gentlemen from across the Atlantic and from the West Indies has emphasized the fact that there are no national lines in science, that we are working together for the same object, and there are none in the world more truly brethren than those who are united in the bonds of scientific investigation. These gentlemen, coming to us as they have, have added very greatly to both the pleasure and the importance of this conference. They have given us very valuable contributions, and I am sure we all feel that there could not possibly have been a more admirable opening of the sessions of the conference than that which Professor Bateson gave in his admirable address on Tuesday morning. I simply wish in this informal way to express our appreciation of the attendance of these gentlemen, and desire on behalf of all of us to give them our thanks for not only their attendance, but their very valuable contributions to the work of the conference.

W. BATESON: I should like on behalf of those who have had the honor of taking part in this conference at the invitation of the Horticultural Society of New York to express in some imperfect way our thanks and our appreciation of the trouble they have taken in getting us together. I came with high hopes of what I was going to hear, of the enjoyment that I knew this conference must be to me, but I must say this very far surpasses the very high hopes that I had formed. To have been in contact for these few days with the number of persons we have met here who are so keenly interested in this business of plant breeding and the detection of the fundamental truths in the life of plants, has been more stimulating and inciting to my mind than I can describe. I knew that very great work was going on in America, but what is going on is far greater than I had any knowledge of. I have met some of the gentlemen from Washington, and I hope in the course of the next week to see something of the work that they are doing there; but I can

see that though in England we had some idea of the work that was going on, we had no idea of its comprehensiveness and magnitude. We try to feel proud of our country, and we always succeed more or less.

While we appreciate what others are doing, we must not too greatly belittle what is going on in the Old World. And it is with great pleasure that I say that I believe the first conference of this kind, though on a much smaller scale and in many ways less important than this conference, did take place in London, and I believe the germs which have grown and developed into such an important undertaking were laid possibly at that meeting of the Royal Horticultural Society in London.

The rapidity with which these processes of fertilization go is most wonderful. We have hammered rather hard at the progress that has come about in science and horticulture through discoveries. I don't know that we have heard too much of that, but we have heard a good deal. A gentleman said yesterday that the Mendelian law was not a law, that it was not true; but it has this merit, that it is the only law that we have as yet on this subject, and, like a great many other things, it is growing very fast, and I believe it will do much more for us in the future. Yesterday was so full, owing to the national genius of the country, that a gentleman said it was worth a thousand dollars. I would not put it at such a high figure, but it is worth a great deal.

If you fancy yourself in a new country, especially in a new city, trying to find your way about; there are some cities the names of whose streets are put up; in other cities they are not put up, or not very conspicuously put up. In such a city as that, think of the traveler trying to find his way; he hunts about for the names of the streets and never finds them; but when it dawns upon him that the streets are arranged in any particular order, that there is not a confusion, that through three-fourths of that city at least the streets are all in arithmetical order, all rectangular and all arranged so that he knows where to look for a certain number, there is an illumination,—that is what Mendel's law does always for the student. When it first dawns upon him that there is regularity, and the further he goes the more he will find this, the feeling in his mind is not less acute than is that of the traveler who discovers that there is order in the streets of New York.

There remain, of course, in our country, just as in New York there remain, those extraordinary complex streets, the growth of time, matted together, tangled up, that we cannot see our way through; but, having found our way through a great piece of it, we are confident that in time we can find our way through the whole region. I should like to express in the warmest way our thanks to the officers of the society, to you, sir, and to Mr. Barron, and to Dr. Britton, for their kindness in bringing us together and giving us the opportunity of mind meeting mind, as we have done in the last few days, and I am quite sure that if such meetings are continued, as I feel confident they will be, in the future, that we shall see very great results come from them.

D. Morris: I have very great pleasure to second the remarks made by Mr. Bateson as representing one section of the visitors to this conference. It has been to me the greatest possible pleasure to come here and to take part in the conference. When it was first suggested that I come, I was afraid

that the distance was too great and that the subjects that would be brought forward to the conference were such as would not be of great assistance to workers in the West Indies.

But since we have been here, Mr. Fawcett and myself have been thoroughly delighted with the business that has come before the conference, the thorough earnestness with which the subjects have been dealt with, and with the large amount of valuable information that we have received.

The information itself has been most valuable, but also we have had suggestions given to us with regard to our future work that I believe will be of the greatest possible benefit to the West Indies. To you, sir. and to members of the council, and to the officers of the Horticultural Society of New York, I have the very greatest pleasure in seconding the sentiments proposed by Mr. Bateson.

W. SAUNDERS: I heartily concur with what has been said by the two previous speakers as to the importance of this great international gathering of those interested in plant breeding, and as to the great pleasure which it has afforded us all to be here. And, also, may I join them in the expectation that great good will come of the deliberations which we have been fortunate enough to be here to take part in as representing the country to the north of you? It is not a small country, either; a country we are rather proud of; as to its size we have a good deal to be proud of, and we hope some day to get it filled.

In that country there are a great many problems relating to horticulture and agriculture, problems which can only be overcome in the long run by the plant breeder, and this international gathering, which we have had the pleasure of taking part in, is one which I consider of very great and particular value to us who live in the North. There are many districts of our great country, which is destined to be the home of many millions of people, where the products which we now have and which we can grow so successfully in the central parts of the continent cannot be cultivated at all; they do not grow, they will not stand the various changes that occur in the climate and the extremes they have to meet.

Now, these difficulties can only be overcome by plant breeding. Find some plant which will succeed there,—however low it may be down in the scale,—and then, by working in these different elements of which Mr. Munson and a number of other speakers have told us, we hope to be able to build up character in these products. We are trying to do that, and thus in time prepare for cultivation in these districts, which naturally seem so unfitted for the work, such products as will be useful and profitable to our people. I cannot very well express the pleasure and gratification I have had—it has been so great—in attending this meeting, and I am sure I only voice the sentiments of my confreres, who are here from different parts of Canada, that we all feel we owe a debt of gratitude to the officers of the Horticultural Society of New York for inviting us to be here, and we are all delighted that we came.

H. H. GROFF: Speaking from the ranks of the unscientific workers, I would like to record my great pleasure at the honor and privilege that I have felt it to be to be with you during this conference. I shall always look

back to it as one of the brightest experiences of my life, if I never have the pleasure of repeating it, as I hope on some future occasion to do. And I would like to say further that I think that it is very fitting indeed that the initiative has been taken by that nation of which it has been said it holds the highest hopes of humanity.

G. NICHOLSON: May I trespass upon your time just for a moment? I should like to express my gratification and the pleasure I have taken here to-day. The establishment of which this forms the center is an establishment worthy of a great city and a great country. Such an establishment as that did not exist when I was in America before, and I am surprised at the developments of a few years, in the developments of which any nation might well be proud. I was here in the States about nine years ago, and the number of people and the hustle of every one impressed me very much then. I believe it is still more pronounced now. An old friend of mine once paradoxically said that horticulture must be intimately related through the blood of plants and through botany, purifying and improving it, with goodness, and a man could not help being better. Judging from my experience, the importance of the horticultural society cannot be overestimated. I shall always remember this conference with the greatest of pleasure.

W. M. HAYS: I wish to say a few words representing in a way one class of the people at least who are working along this line of the improvement of our plants and our animals in a somewhat organized way, in our great system of agricultural colleges and experiment stations, including that greatest one at Washington. I am only one of two or three who were in the conference three years ago in London. There has been great progress since that time, and the substance and subject matter of this meeting is a substantial evidence, a most remarkable evidence, of the progress that is being made in a number of ways, and especially of progress in the interest taken in this subject, both from the practical standpoint and from the theoretical.

We have come to believe that the theory of plant and animal improvement along scientific lines can be worked out, not only for the past, nature's way of working it out, but for the future; and the people have come to believe as never before that this great question can be approached in a large way and much done for the good of humanity. Some of us after the meeting at London thought of organizing along these lines more carefully, and a great deal of that organization has already taken place. The Department of Agriculture at Washington has in hand, and already established, certain features of organization bringing about co-operation between the experiment stations and also the government with groups of individual experimenters.

Secretary Wilson has a thorough appreciation of this whole matter, and has done a great work in getting it in form where it is going to bring about great financial results for this country. No doubt there will be meetings of this kind of an international character again. When we were at the banquet in the Horticultural Club in London, a meeting similar to this, I remember, it was suggested that we might some time meet in Paris, and Mr. H. de Vilmorin, who is since dead, was in hopes that he might be instrumental in bringing about such a meeting. We were so delightfully entertained in London that

some of us, at least, will be anxious to go to the other side of the water again some time.

THE CHAIR: We have all of us had varied experiences, possibly, in the entertainment of guests, and varied experiences in being guests ourselves, and we know that the relation is never perfectly satisfactory unless there is evidently mutual enjoyment, when both host and guest thoroughly enjoy the association one with the other. It did not require the words that have been spoken on this occasion for us to know that this condition of things existed. Still, it is very fitting that the expression should have been made, and we of the Horticultural Society of New York accept with very great appreciation the expression that has been given by our guests who have been present with us for these days.

RESOLUTIONS

W. B. Alwood presented and moved the adoption of resolutions expressing the thanks of the conference to the Horticultural Society of New York and to its officers and to the managers of the New York Botanical Garden. The motion was seconded by C. L. Watrous, and carried.

C. L. Watrous, as chairman of the committee appointed to consider the matter of co-operation between practical plant breeders and the United States Department of Agriculture, presented and moved the adoption of the following resolutions:

Whereas, This conference recognizes the invaluable services which the United States Department of Agriculture is rendering to the breeders of plants and animals by collecting and publishing the results obtained by workers in these fields throughout the civilized world; and

Whereas, We believe that plans for a still closer co-operation can be arranged to the mutual advantage of the department and individuals, associations and institutions interested in plant and animal improvement; therefore, be it

Resolved, That a committee of three be appointed by the Chair empowered and directed to confer with the Honorable Secretary of Agriculture with a view to the formation of plans for the more intimate co-operation in future between individual workers and the department in question and publishing data relative to plant and animal improvement.

The motion was seconded and carried.

The Chair appointed as members of the committee provided for in the resolutions the following: C. L. Watrous, W. M. Hays and L. H. Bailey.

After an announcement by Dr. N. L. Britton regarding the inspection of the grounds of the Botanical Garden by the visitors, the conference adjourned.

APPENDIX

PAPERS PRESENTED

TO THE CONFERENCE AND

READ BY TITLE

NOTES ON THE BREEDING OF BEANS AND PEAS

By W. T. Macoun, Horticulturist, Central Experimental Farm, Ottawa, Canada

The following notes of an experiment to determine whether the time of maturing of beans could be lessened by selection may be at least suggestive. The experiment proves that even in the first year there was a marked difference. Reference is also made in these notes to a pea cross and the selection which followed where the results proved how readily once a cross had been made, the size of the pea could be altered by selection.

BEANS

In the year 1899 the earliest ripe pods from the earliest ripe plant of the variety of bean known as Challenge Black Wax were selected. The beans from this pod, six in number, were planted in 1900. There was a difference of three days between the time of ripening of the earliest and latest plant of these six. The earliest pod was taken from the earliest plant, and the beans from this, five in number, were planted in 1901. There was two days' difference between the earliest and latest plants. All the pods from the earliest plant were saved and twenty beans sown in 1902. This year there was six days' difference between the earliest and latest plant. Only seven plants out of the twenty had beans ready for table use at the earliest date; one, two days after; nine, three days after, and three, six days after.

In 1901 after selecting the earliest ripe pod from the earliest of the five plants growing, the rest of the beans were saved and sown in 1902, when the beans were three days earlier than seed freshly imported from Philadelphia. In 1902 seed from these was saved and sown alongside freshly imported seed from Philadelphia. The crop from our seed, though the beans had gone one year without selection, was seven days earlier. On different soil there was a difference of six days. How much of this difference is due to climate and how much to selection has not yet been determined, but that much of it is due to selection is indicated by the fact that among the selected plants themselves there was a difference of six days between the earliest and latest.

PEAS

Many crosses have been made at the Dominion Experimental Farms between different varieties of peas. In the year 1892 a cross was made by Dr. A. P. Saunders with Black Eyed Marrowfat as the female and Mummy as the male parent, the former being a large, black-eyed pea and the latter a medium sized, white one. The peas from the crop of that year had black eyes and resembled the Marrowfat very much. These were sown in

1893 and the yield from each kept separate. In 1894 the crop from each was sown again, and each was harvested separately as before. That year both the vines and peas showed marked variations. There were vines with the flowers in clusters at the top like the Mummy, and other vines with the flowers scattered. There were large peas with black eyes, and small peas with black eyes, small, medium and large pure white peas, and peas with a yellow or dusky tint like the Black Eyed Marrowfat, but without the black eye. A single plant having flowers in clusters at the top, but having peas with black eyes, was selected and the crop saved. The largest peas and the smallest ones, which were smaller than any known named variety, were selected from this crop and sown separately in 1895. The difference in size of the peas produced from these was very marked. Most of the peas from the small seed were quite as small as the peas sown, and on the other hand the peas from the large seed were as large and larger, and in 1896 the results were the same. Selection has not been carried on regularly with these since, as sufficient evidence was obtained of what could be done in selecting for size in peas once new blood has been introduced by cross-breeding. As far as the writer is aware, experiments have shown that it requires a long time to make any marked difference in the size of an established variety of pea by ordinary selection.

Experiments have been in progress at the Central Experimental Farm for the past three seasons in the selection of named varieties of peas with a view to increasing the number of peas per pod, and productiveness of the plant, and to lessen the number of days in maturing. No marked results have yet been obtained.

IMPROVEMENT OF CORN BY BREEDING

By C. P. Hartley, U. S. Department of Agriculture, Washington, D. C.

Perhaps in no other way can plant breeders be of so much benefit to the country as by creating reliable sources from which farmers can procure pure and superior seed corn of varieties adapted to the soil and climatic conditions of various sections. Because of the difficulty of preventing admixtures and also because so small a quantity of seed is required per acre, growers on small farms will probably always find it to their advantage to annually buy their seed corn, if they have reliable sources from which to obtain pedigree seed of sorts adapted to their respective localities. In a few sections of the United States strains of corn have been originated and acclimated, and from these small quantities of seed adapted to those sections can now be obtained; but the greater part of the corn-producing area of this country is without sources from which to obtain seed corn of good quality. Without other additional expense or work, the substitution of good pure seed corn for that now planted throughout the country would increase our corn production by millions of bushels.

The time has not yet come when general advice can be given to growers regarding the purchase of their seed corn because the sources of supply are not yet numerous enough, nor is the quality of seed that can be purchased as good as it should be. Furthermore, there are too many unscrupulous dealers who, by misrepresentation and exaggerated statements, are ever ready to sell at high prices corn that has been bought at market price and that possesses none of the requirements of good seed corn.

It is, however, in order to advise all those who are capably situated to begin at once, and by careful breeding, to fix a pure type of corn, suited to the soil and climatic conditions of their sections. This would by no means include all corn growers, for the farms of many are so small that they cannot give a corn the necessary isolation, and if their farms be of average size, proper crop rotation will bring their cornfields too close to those of a neighbor who grows poor corn. There are but few in each State who have ideal conditions under which to breed a pure corn, and it therefore behooves plant breeders to produce stock seed of pure strains from which the growers in any corn section of the country can buy first class seed adapted to their farms.

The corn crop of the United States represents more money value than that of any two other crops, and this is the leading corn country of the world; yet no intelligent grower is willing to say that no improvements in corn are needed. Let us consider (1) what improvements are needed; (2) whether their attainment is possible, and (3) the best means of accomplishing them.

In respect to yield there is much room for improvement. The average yield per acre in the United States is scarcely one-fourth what it might and should be, and, strange as it may seem, it is not greatest in the sections where corn naturally thrives the best. For instance, an average of the production in each of the New England States for the past ten years is 36.49 bushels per acre, while the average production in Illinois during the same time is 31.55 bushels, and in Iowa, 30.93 bushels per acre. This must be due to the fact that in the New England States much more care is given to the few acres that are planted, than to the large cornfields of the States of Illinois and Iowa, for one can scarcely believe that with the same care in all respects as much corn can be grown on an acre in Vermont as on an acre in Illinois. Grouping some of the States according to their geographical situation and averaging their corn production without considering the acreage of the various States, we find the average number of bushels per acre for the ten years from 1892 to 1901 to be as follows: New England States, 36.49; New York, Michigan, Wisconsin, Minnesota, South Dakota and North Dakota, 27.6; New Jersey, Pennsylvania, Delaware, Maryland, West Virginia and Virginia, 26.97; Ohio, Indiana, Illinois, Iowa, Nebraska, Kansas, Missouri and Kentucky, 27.38; South Carolina, Georgia, Alabama, Mississippi, Louisiana and Texas, 13.59. It will be noticed that the group containing Ohio, Indiana, Illinois, Iowa, Nebraska, Kansas, Missouri and Kentucky, the most extensive corn producing States of the Union, give an average of but 27.38 bushels per acre, while the average of the New England States is 9 bushels greater. The general average for the entire United States for the ten years from 1892 to 1901, inclusive, is but 23.51 bushels per acre. While this low average is to some extent due to lack of attention in various respects, such as cultivation, etc., it is also in a very large measure due to the poor quality of seed corn that is planted throughout the country.

Much credit is due to individual growers in certain sections or counties of various States for instituting methods of seed selection and culture which have proved so satisfactory that they have been adopted by many of their neighbors. This accounts in a great degree for the usual good crops in some counties, while other counties, equally as fertile, produce much lower averages. In some sections, noted for their large acreages and many bushels, the corn is of very poor appearance and quality because the effort is to grow as many acres as possible rather than to grow the best corn and much of it to the acre. At one of the great corn shipping centers of the United States, a heavy corn buyer made this remark: "Our corn looks a great deal better after it is shelled than it does in the ear."

The secret of improving yields lies in causing every stalk to produce abundantly, and this is accomplished by planting seed from stalks that produce well, and which also have the power of transmitting their productiveness to their offspring. Valuable strains of this kind can be obtained only by persistent effort. Exact records greatly facilitate this work.

In working to increase production, much attention must be given to the quality of the crop produced. Eighty bushels per acre of ear corn that yields 80 per cent of shelled corn is not as desirable as seventy-five bushels per acre of a corn that furnishes 90 per cent.; nor is seventy bushels of shelled corn

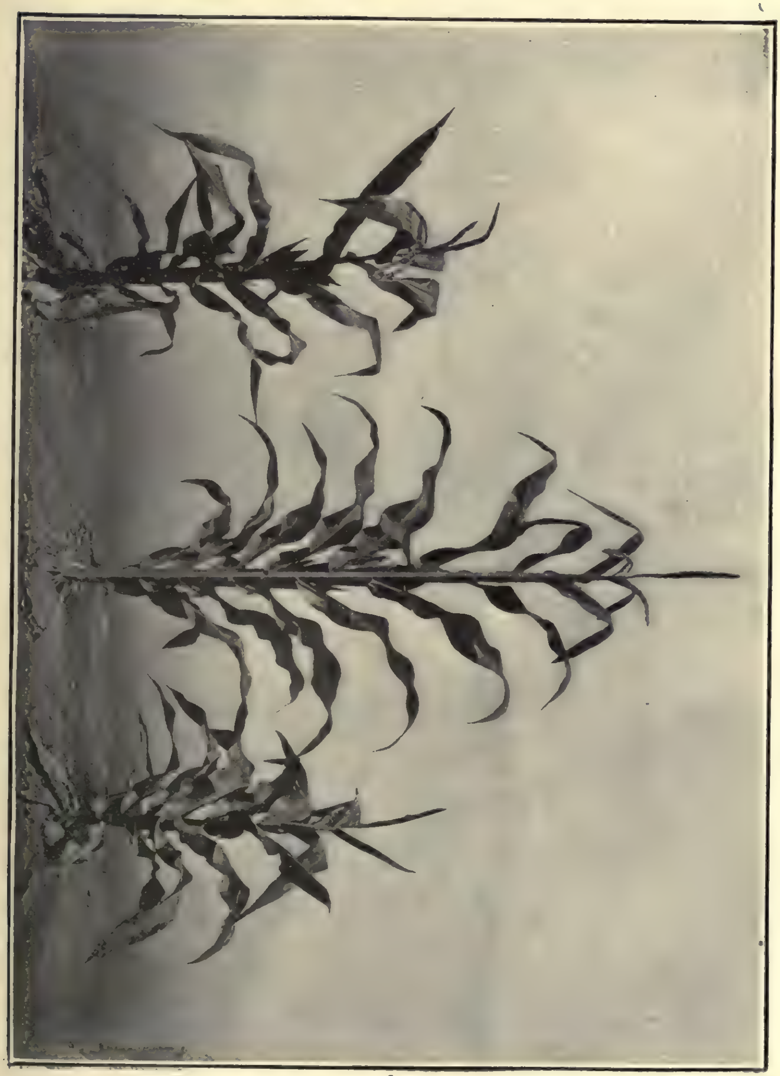


Fig. 1.—Shows leafy stalks noticed in a field of white dent corn.

Fig. 2.—Corn plants shown in Fig. 1 with shoots and tassels enclosed in paper bags.

per acre of a corn low in oil and protein as desirable as sixty bushels of a corn high in oil and protein.

While great productiveness of shelled corn, rich in feeding value, is the leading qualification for a corn to possess, it is folly to suppose that any one strain possessing this qualification to a superior degree would displace all other corns and thus eliminate all danger of injurious cross-breeding. We must, and will, always have various types of corn. We must have different corns for various geographic sections, and we must have different corns for various purposes, and in order that satisfactory returns may result from our labor, we must hold these various types to a high degree of perfection and prevent admixtures. We must have sweet corns early and late, rich in sugar, for the table; we must have popcorns rich in volatile oils, for the children; we must have corns with large flinty kernels for hominy and grits, rich golden corns for some markets, and pure white ones for others; and we must have corns with rich, succulent stalks and foliage, for the silo.

Besides these essentials, there are other points that need the corn breeder's attention. The cattle feeder demands a prolific corn, but the ears must not be large and the kernels and cobs must be soft enough for the animals to masticate. In southern sections it is usual for the stalks to grow too tall, with the ears too high to be reached conveniently, while in the North, the corn breeder must encourage the production of ears higher on the stalks so that there will be room for the harvester to cut the stalks below the ears. The length and size of ear stalks need the attention of the breeder. The peduncle, or stalk that bears the ear, should be long enough and slender enough to be bent by the weight of the mature ear, so that the latter will assume a pendent position, and thus remain dry during rainy weather. A very long ear stalk is a waste of growth. The husks should protect the ears well from insects and water, and yet not be so tight as to prevent the development of the kernels nor interfere with the ease of husking. The form and appearance of the ear is already receiving much consideration by those who are giving attention to corn breeding. A cob of a given size should support as many kernels of uniform size and shape as possible. This is accomplished on cylindrical ears with straight rows and well filled ends.

All who have had experience in breeding plants will admit that the needed points of improvement that have been briefly pointed out are possible of accomplishment. A few specific examples will now be given to illustrate how readily corn responds to selection of seed for the purpose of modifying either stalk, ear, or kernel.

In 1901, in a field of tall growing, white dent corn, a few short and very leafy stalks were noticed (Fig. 1). The figure 2 shows the same plants with shoots and tassels enclosed in paper bags. These stalks were but four or five feet tall, and bore from eighteen to twenty broad leaves, while the other stalks of the same corn were 10 feet tall, but bore fewer leaves. A few adjacent tall stalks were removed and the short ones cross-pollinated by hand one with another. The seed resulting from these short plants was planted in one row in 1902, in a field planted with seed from the normal tall stalks of the same corn. How very much the stalks in this one row resemble the parent stalks is shown by figure 3. The difference in the

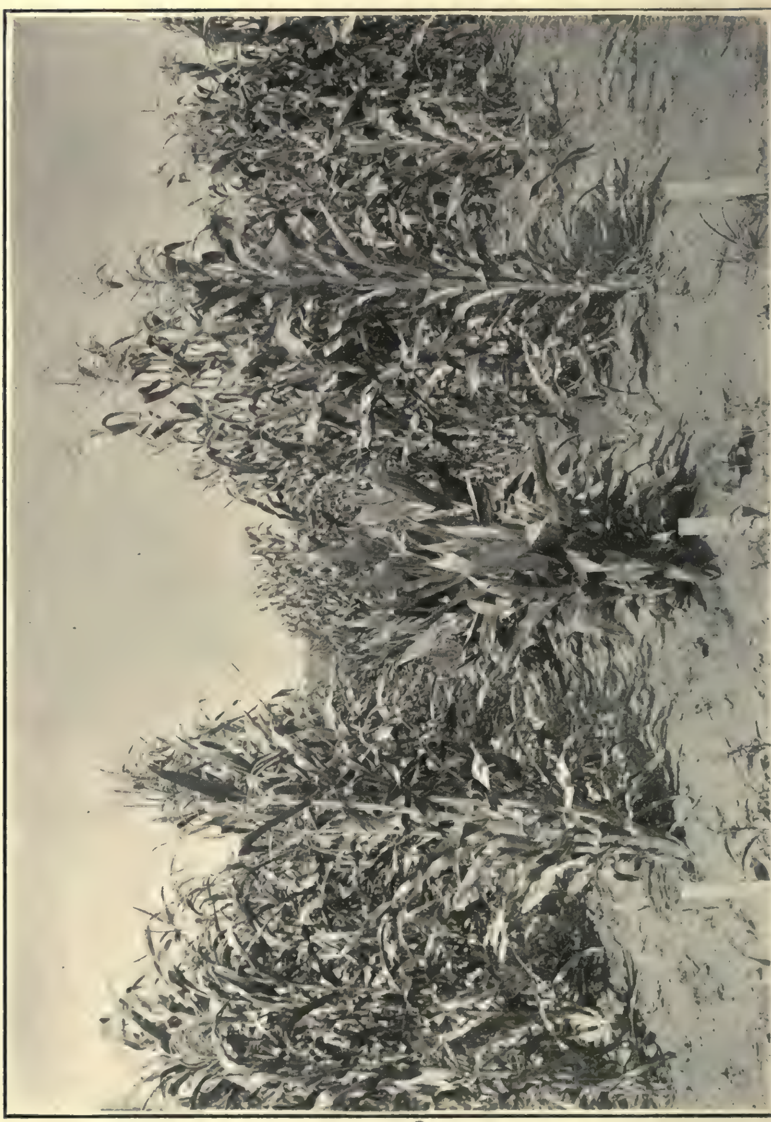


Fig. 3.—Selected dwarf corn stalks shown in the center row. One year's seed selection.

appearance of the stalks in this row from those seen in the adjacent rows is the result of one year's seed selection with reference to the characters of the parent stalks.

The need of giving attention to the ear in selecting seed corn may be seen in the following record of seven individual seed ears of a pure corn. The ears were planted separately on uniform soil and tended similarly in all respects.

Length of ears planted.	Average length of 30 good seed ears harvested.	Average weight of all progeny ears.	Average pounds of ear corn per stalk.
8 inches.	8 1-2 inches.	.565 pounds.	.550 pounds.
8 "	9 1-5 "	.554 "	.572 "
8 "	9 1-3 "	.467 "	.619 "
9 "	9 1-10 "	.514 "	.582 "
9 "	9 1-3 "	.584 "	.598 "
10 "	9 1-2 "	.583 "	.699 "
10 "	9 5-6 "	.587 "	.688 "

That the separate kernels of a seed ear have different transmitting tendencies is illustrated by the following. In the spring of 1901 a very unusual

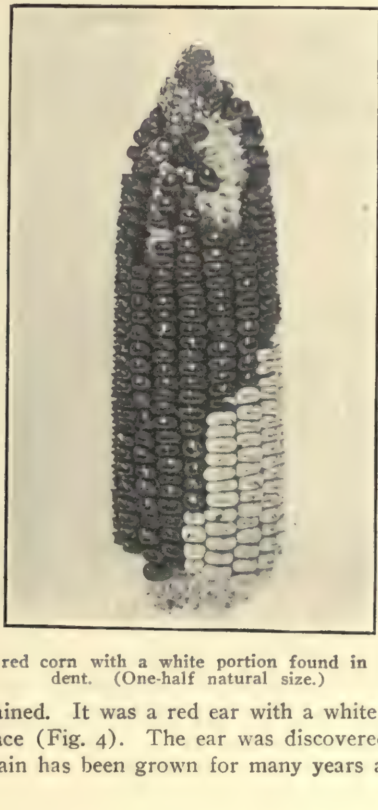


Fig. 4.—Ear of red corn with a white portion found in a field of white dent. (One-half natural size.)

ear of corn was obtained. It was a red ear with a white spot covering about one-fifth of its surface (Fig. 4). The ear was discovered in a field of white dent corn. This strain has been grown for many years as a reasonably pure

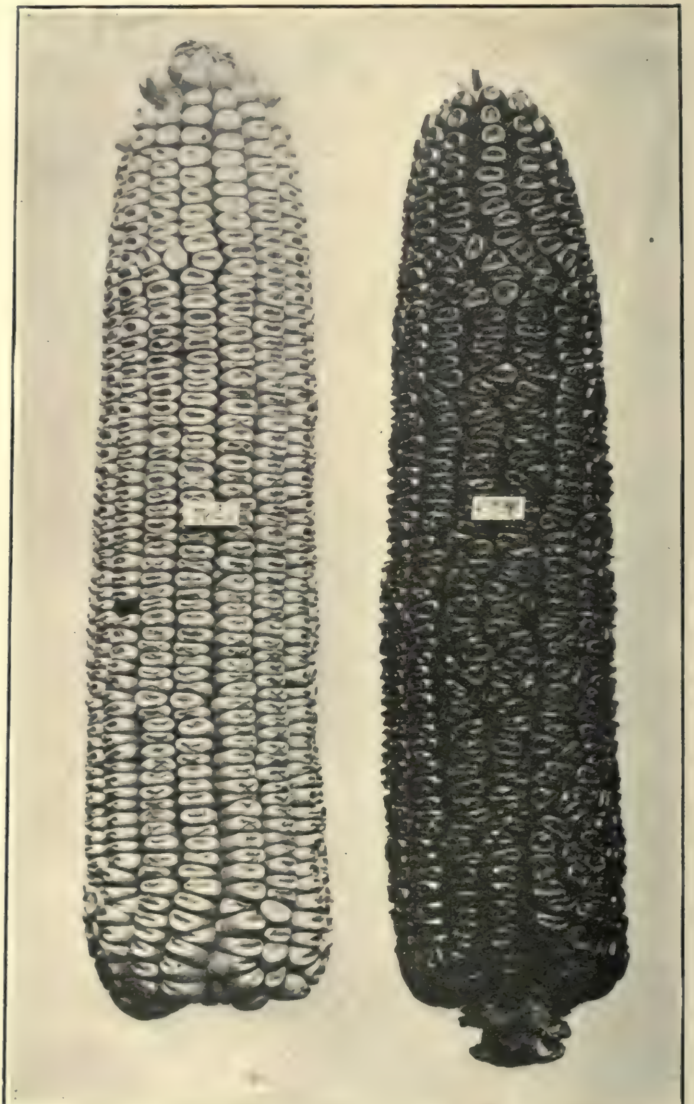


Fig. 5.—Grown from the red kernels seen in Fig. 4. (One-half natural size.)

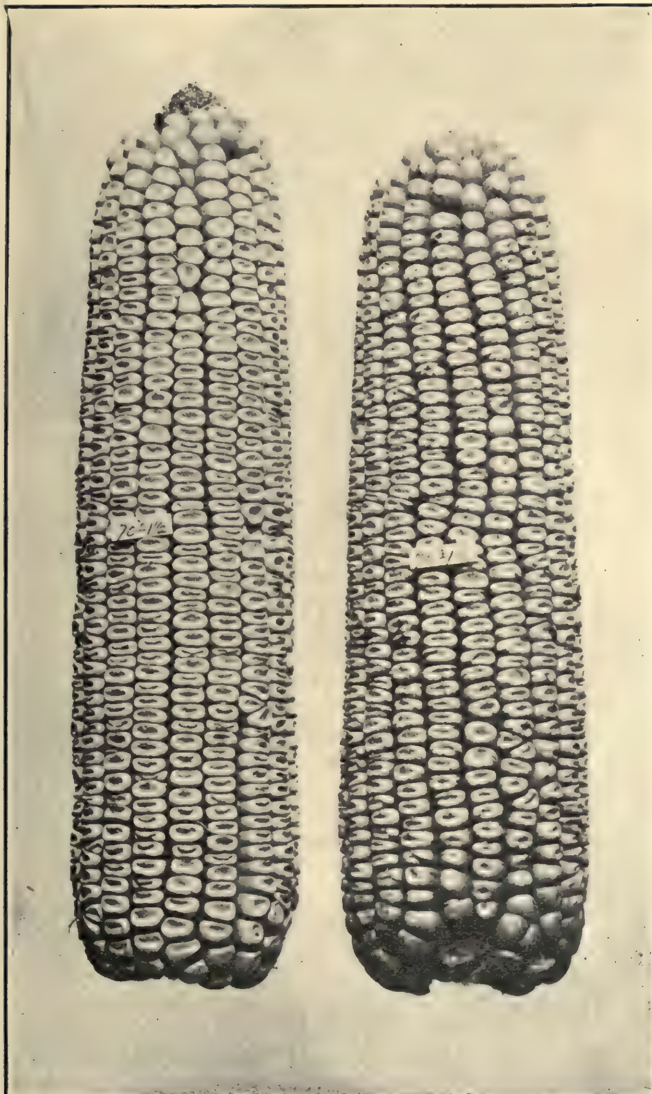


Fig. 6.—Grown from white kernels seen in Fig. 4. (One-half natural size.)

white dent, which occasionally produced a red ear, but was never before known to produce ears bearing spots. The red kernels of this ear were uniformly red with a small light spot at the cap of each kernel. The kernels composing the white spot, although appearing very white in contrast with the red ones, when examined closely proved to have very fine red lines or streaks, radiating from the caps down the sides of the kernels. These two types of kernels were planted separately. The red ones yielded a crop composed of eighty-four red ears and eighty pure white ears; while the white kernels with fine red stripes produced thirty-nine ears with kernels like those planted, and thirty-six pure white ears. In the illustration is shown the red ear with the spot at the base. The two ears labeled 70-1 represent the pure red ears and the pure white ears that were produced in about equal quantities by planting the red kernels of the spotted ear. The two ears labeled 70-1½ represent the pure white ears and the ears bearing striped kernels like those from the white portion of the spotted ear.

It should be borne in mind that these special cases cited are illustrations of the selection of individual variations, and not variations purposely produced by hybridization. They are given, not because they show progress in desirable lines of corn improvement, but because they illustrate how readily the corn plant yields to modifications of stalk, ear, or kernels, through seed selection, in accordance with individual variation. The conclusions that necessarily follow are these: If stalks with certain qualities are wanted, seed must be taken continuously from such stalks; if ears with certain qualities are wanted, seed must be taken continuously from such ears; if kernels with certain qualities are wanted, such kernels must be planted; and furthermore, if stalks with certain qualities, bearing ears with certain qualities, containing kernels with certain qualities, is what is wanted, attention must be given to stalks, ears, and kernels when selecting seed. To fulfil these requirements seed ears cannot be selected from a corn crib, but must be selected from standing stalks. Every seed corn grower should have a seed patch and an *increase* patch. In the seed patch the very best ears should be planted, each row being planted with seed from a single ear. The crop from each row should be weighed so as to obtain the performance record of the seed ears. From the best rows a few of the choicest ears should be taken and used in planting the seed patch the next year, while the other good seed ears can be shelled together and planted in the *increase* patch, which should be of sufficient size to yield enough good seed for planting the general crop.

The many needed improvements, the possibility and method of their accomplishment, have been briefly discussed without speaking of hybridization. The possibilities of corn improvement are so great and are so certain of accomplishment by isolation and rigid selection that little need be said concerning hybridization in this connection. For one with an abundance of time for keeping records, the corn plant offers one of the best fields for the study of the problems of heredity. While the plant breeder has learned much and will learn much more by corn hybridization, the corn grower has improved his corn much and will improve it much more by seed selection.

Breeders make use of hybridization to cause increased variation of characters, but for the present this is not necessary in the case of corn. We now have many types each exhibiting variations to a high degree, so that for practical results the augmentation and fixation of desirable characters is far more important than is an increase in the number of characters.

MY EXPERIENCE IN HYBRIDIZING CANNAS

By Antoine Wintzer, West Grove, Pa.

It is about nine years since the writer first commenced to experiment with cannas, with the object of improving the strain and creating some new and desirable varieties suitable for our trying climate. At that time we depended almost entirely on the skill of the European growers for our novelties in cannas, and they sent us annually a great many new varieties. While some of these novelties were good, a great many were little, if any, improvement, on existing varieties.

After growing a few seedlings from the best strains, the writer commenced to cross breed with the intention of producing a good solid yellow canna. There were plenty of spotted yellows, but we desired something purer. In 1893, from a batch of Crozy and Star-of-1891 seedlings, I was fortunate in getting one almost yellow. It was named Golden Star. The next year I succeeded in growing from another lot of seedlings another almost pure yellow; it was named Coronet.

By crossing these two varieties I succeeded in producing Buttercup. This variety seems to have the desirable qualities long looked for in a yellow canna. It is rather dwarf, an early and free bloomer, erect head held well above the foliage, endures the sun without bleaching, drops its faded flowers, which always gives it a bright and clean appearance. It will also bloom under a lower temperature than most varieties, and last, but not least, its tubers are small and solid, making it especially valuable for pot culture. Besides the yellow, I was also desirious to grow some good pink varieties.

To enable me to get these I had a good start with Pink Ehmani, which I raised in 1894 from seed hybridized by Dr. Van Fleet. Having a start in color, I hybridized it with other varieties, and produced Maiden's Blush, Rosemawr, Martha Washington, Betsy Ross. The main difficulty found in the varieties of this color was the poor keeping quality of the tubers. In the earlier varieties they were soft and spongy and liable to rot in a dormant condition, long before the weather was warm enough to plant them in spring. The last two named varieties are free from this bad habit. They usually produce small hard tubers of good keeping quality.

After breeding cannas for a few years, I noticed that it was desirable to produce small and solid tubers. A great deal of this work is still in its infancy, but we are slowly advancing along that line. In the early '90s there were several good red cannas in commerce, and any one at that time looking over the leading catalogs and reading the description of such varieties as Alphonse Bouvier, would wonder how a more brilliant color could be produced, and I often longed for the shade of red we had in such roses

as Prince Camille de Rohan and Baron de Bonstettin. In the production of Philadelphia and Pillar of Fire, I became hopeful, and more so when later, Duke of Marlborough, Black Prince and Cherokee came into existence through my efforts along that line. The Duke had the most interesting history, as being the production of a very inferior seedling, which had nothing to recommend it except its dark color. Its pollen used on Philadelphia produced the Duke of Marlborough. In working for solid color, I managed to produce a great many shades and combinations of colors found in such varieties as Lorraine, Niagara, Conqueror, Schley, Duke of York, Striped Beauty and a host of others, most of which were thrown into the mixture after they were tested for a couple of seasons. Alsace, the nearest to a white canna, although small, was useful in massing, and is now extensively disseminated. It was produced in 1894. From its pollen I produced Montano, Starlight and quite a number of seedlings of little value, and only useful for breeding. At last I produced one which proved superior, and it was named Mt. Blanc. It was almost pure white, with full-sized flowers produced on strong, vigorous, erect stalks, carried well above its rich, massive foliage. The habit of plant is vigorous and of good constitution.

In the Canna indica section we had very little variety in colors. After crossing these for several years, I produced Mt. Etna, Queen of Holland, Shenandoah, Evolution. These are giving us a wider range of colors. The last named is proving very hardy and vigorous. Its odd color, a blending of orange, salmon and yellow, making a fine contrast with its rich bronze foliage. To produce the different colors and types mentioned, it was necessary for me to do a considerable amount of hand hybridizing. This work was done at odd times when conditions were favorable, generally in early morning. We usually plant from four hundred to five hundred of these seedlings in the field annually in June. The seed is started under glass in April, and germinates quickly. When they show two leaves they are potted into 2½ or 3-inch pots. The majority of them bloom in August. At that time I always look over them daily and number or mark the most promising ones.

In reviewing the work of the past I find that the mistake made is in numbering too many. I find that it is well not to do much of this work on cloudy days, as under such conditions cannas of average quality show up well. For several years I have selected hot, dry days, from 1 to 5 o'clock p. m., with the thermometer anywhere from 90 degrees, up, in the shade. Under such conditions it is necessary for a canna flower to have substance to make a show.

The work of selecting seedlings is becoming more difficult, as there are several expert canna hybridizers in Europe and in this country who are working and developing fine novelties, and we are all striving for the ideal canna, with the hope of producing it in the near future. Any one looking back a decade will admit that the work of the canna hybridizer has developed the flower to a remarkable degree, and the canna of the present time is worthy of a place in the finest conservatory, and in the near future will be used as a decorative plant. The canna has not received the attention it justly deserves. Nearly all our public parks are planted with inferior varieties. If these were thrown into the dump pile, and their places filled with the improved varieties, the public would have a better opinion of the canna as a blooming plant.

In conclusion, the writer would say that the labor of the hybridizer is not so arduous as some would have us believe. Why should he care if the dew is wet, or the sun hot; is he not laboring for love? Is it worth nothing to watch a plant grow and thrive under your care and produce its beautiful flowers for your eye to behold?

HYBRID PLUMS

By F. A. Waugh, Dept. of Horticulture, Massachusetts Agricultural College, Amherst, Mass.

A large number of hybrid plums has appeared on the scene in America during the last few years. They have aroused considerable interest, and many of them have been rather extensively planted. There is, therefore, a good deal of information extant regarding their behavior and a healthy demand for further information. Such matters as have come within the scope of my own observation and experiment have been presented in various articles—principally in the publications of the Vermont experiment station.

In the papers referred to I have described a considerable number of varieties, old and new, and have given the historical data regarding their origin. In many cases the question of parentage and affiliation has been discussed. It has been shown, also, that various species participate much more frequently in garden hybridization than do others; and various suggestions have been made in connection with these observations for the further breeding of plums.

One observation which has been repeatedly made, and which I wish to make the principal point of this discussion, relates to one of the fundamental laws of hybridity. It has usually been laid down as true that offspring of the first generation from two specific parents will greatly resemble one another. There is likely to be a wide range of variation in the second generation, but the first crosses are said to be usually very uniform. In plants propagated by buds this uniformity will, of course, be preserved by grafting or similar means, and it becomes more interesting on that account.

The general law quoted above seems to be verified among the hybrids already produced in the genus Prunus. While the uniformity is not always so striking as would justify the law aside from experience in other genera, still it is sufficiently obvious to be worthy of note. The offspring from any given pair of specific parents seem to be quite as uniform as any of the other common groups among the plums. It is a well known fact, of course, among pomologists, and even among botanists, that all or nearly all our species of Prunus are very variable. Anything like a reasonable degree of uniformity, therefore, in a group of hybrid offspring should be accepted as worthy of remark.

The three species which have been most frequently used in hybridization experiments in this country, and which have also bred most readily with each other in the field, are Prunus hortulana, P. triflora and P. simonii. The most common cross is the one between the two first named. These hybrids have appeared naturally and artificially literally by thousands; and a large number of them have been named, described, propagated and distributed.

The best known varieties of this group, which may serve to recall its characters to mind, are Alabama, America, Excelsior, Golden, Gonzales, Govalle, Juicy, Lannix, Louisiana, Minnie, Monolith, Nona, Preserver, Scribner, Watson, Waugh and Yates.

These varieties are as much alike as those in the hortulana group, for instance. A new variety of this parentage can be recognized quite as easily and as certainly as a pure Chickasaw, a pure Americana or a pure Domestica. For this reason it seemed to me just and proper to describe the group separately and to give it a name.

This description appeared in the fourteenth annual report of the Vermont experiment station, p. 276. Horticulturally the group was called by the name of the variety Gonzales. Botanically it was named Prunus hortulana robusta.

The next most important group of hybrid plums seems to be that which counts for its parentage Prunus hortulana on the one side and P. simonii on the other. In this series Wickson is much the best known variety. Bartlett, Chalco, Climax, Maynard, President and a few others are supposed to have the same parentage, and so far as I have been able to study them in the orchard they show the same general characters. In the forthcoming report of the Vermont experiment station, therefore, I expect to present a name and description for this group. The present article may be considered a preliminary notice of the final publication here referred to.

The group just mentioned will be called horticulturally the Wickson group. The botanical name and description will also be added, the name proposed being Prunus triflora recta.

The next most important group from the horticultural standpoint appears to me to be the one originating from the combination of Prunus triflora and P. americana. Unfortunately there is very little material which can be really presented to the public. Not a single variety, so far as I know, which illustrates the typical characters of this group is now held for sale by any nurseryman. The variety Ames sent out a few years ago by the Iowa Agricultural College is said to have this parentage. While the record of its pedigree is doubtless correct, the variety does not present the characters of Prunus triflora very obviously, but seems to be almost purely P. americana. The variety Omaha, described by the writer in Vermont experiment station report 14, p. 272, is, however, typical, and a study of this variety, along with several others which I have seen, leads me also to give a group name and description to the varieties of this pedigree. The horticultural name proposed is the Omaha group, and the botanical name suggested is Prunus triflora rustica. This group is a very interesting one, and, in the opinion of the writer, has an important future before it on the pomological side.

These matters are brought up here largely because they illustrate the writer's views regarding the treatment of horticultural groups. Wherever such plants have important characters in common, which may be perpetuated indefinitely, it is desirable that they should be classed together. And where these characters take rank along with those which are commonly used to make specific distinctions in the same genus it is wise to treat these hybrid groups just as the natural groups in the same genus are treated. There is

some difference of opinion, I am aware, as to the wisdom of making botanical names for such cases as these, but the writer is one of those persons who believe that it is nothing against a variety or a species that we should know its origin. The fallacy of requiring a man to be ignorant about any particular points regarding his material in order that he may call it a species has often been pointed out.

Much interest has been aroused recently by the experimental work of De Vries. His work is somewhat in the same line as that which is here reviewed; and those who justify him in calling his groups species will doubtless be content to speak of Prunus hortulana robusta, P. triflora rustica, etc. Those who stick to the strictly old-fashioned and conservative notion of species naming may justly take exception to the practice here illustrated.

THE MUSKMELON

F. William Rane, Dept. of Horticulture and Forestry, New Hampshire College, Durham, N. H.

Of the various plants grown in the garden few have been appreciated more than the muskmelon. The muskmelon is not only a delightful and highly valued fruit when well grown, but under favorable conditions is easily raised and brings good financial returns to the grower.

The writer has grown muskmelons for many years, and is impressed with their constantly increasing popularity and the extended area devoted to their culture. Much of the fruit found in the markets, however, is even yet of little value as regards quality, and were it possible to educate growers to grow only the better varieties, those more delicately flavored, our present area would be very inadequate. Poor, flavorless varieties are just as hard to grow, other things being considered, as good ones. Sunshine, however, is believed to be quite importantly associated with flavor.

When sending in this subject to the Conference the purpose was to make an exhausted study of the development of the cultivated American muskmelons and in so far as possible trace out the origin and history of all of our standard, well known and promising sorts. While the varieties themselves have been carefully studied and an attempt at classification made, it was thought by a close investigation into the origin of each variety perhaps sufficient data might be obtainable to indicate more or less the phyla or line of development.

Early in the year I wrote to all of our leading seedsmen throughout the country personal letters asking them for all possible information in regard to their various introductions of muskmelons, viz., date of introduction, description and photograph of the original fruit, where originated, by whom, whether a known cross, and if so its parentage, etc. I also wrote many large growers throughout the country for similar assistance in tracing the origin and history of this fruit. Some of our leading agricultural and horticultural papers assisted me likewise by publishing a notice free of charge asking for similar assistance. Each of the Agricultural College and Experiment Station horticulturists was likewise consulted and asked to co-operate in the work.

From these various sources a very liberal response was received. Many seed firms were frank to say that they had not introduced anything of their own origin, and even most of those offered to the public were not of any definite known crossing, but largely variations or strains which through selection had been brought out. Some were supposed to be crosses from the fact that other varieties which the chance seedling or strain seemed to resemble had been growing near by. Seedsmen, growers and horticulturists united in not only giving me the advantage of their own experience and data,

but in giving addresses of many persons whom they believed might assist me. These addresses were again written, referring me in turn to still others.

It has been interesting in accumulating this data to get the ideas of growers as regards the development of this fruit. Some seem to think there is nothing grown to-day that begins to compare with the olden time varieties, while others see vast improvements.

All authorities seem to agree that the muskmelon doubtless originated in the Orient, and even to-day it forms a staple article among the peoples of Persia, Italy and Egypt.

The muskmelon is very easily susceptible of mixing where varieties are adjacent, and through this means many strains and types have been brought out. The evolution of the cultivated American muskmelon is interesting. One writer claims that Columbus brought the first seed to this country. There are very few instances on record of direct introduction of varieties in earlier times. In recent years the U. S. Department of Agriculture has introduced many which may have a marked influence in the future. The famous Cassaba or Persian variety is said to have been introduced directly from Smyrna to the light soils of lower New Jersey, where it has flourished and become noted for its sweetness and fine flavor. It is believed that most of our thin skinned, finely netted and highly flavored varieties are of Persian origin. The rougher or hard skinned varieties, like those so commonly grown in France, Italy and other European countries and known in this country as cantaloupes or rock melons, may have had a similar origin, but have been developed differently.

The muskmelon most commonly known to Americans is, I believe, of the Persian type. While some cantaloupes are grown, and there are now and then varieties showing the cantaloupe characteristics, they nevertheless do not meet with as great success.

There has been more attention given to breeding the muskmelon during the past ten years than ever before. While many of the earlier varieties are still standard, new ones are rapidly taking their places. As with other European fruits, conditions have so changed under the new environment in this country that our present cultivated muskmelons are what may be termed strictly an American product.

After much correspondence the data at hand seem very inadequate. It was thought that a large number of our varieties would be found to be of known origin, but they are not. Very few varieties are of known parentage. The varieties are largely chance seedlings or strains of well known varieties.

Much general information was obtained through the two large wholesale seed firms of Chauncey P. Coy & Son, proprietors of the Elkhorn Valley Seed Gardens, and Frank T. Emerson, general manager of the Western Seed and Irrigation Company, both of Waterloo, Nebraska. These firms are large wholesale growers of muskmelon seeds, and largely supply these seeds for our large seed houses. When any of the seed firms have obtained or originated a new variety, they turn the same over to these firms for future supplies.

The writer is unable to tabulate the data as completely as possible, but offers the following as a beginning toward that end;

Variety.	Origin.	Introducer.	Year.
Extra Early Hackensack	Selected Hackensack	Peter Henderson & Co.	
Rocky Ford	" Netted Gem	Rocky Ford Growers.	
Osage	" Miller's Cream	Vaughan's Seed Store.	
Surprise	White Japan × Orange Christiana	Price & Knickerbocker	1876
Bay View	Cassaba × Large California	W. Atlee Burpee & Co.	1877
Chicago Market	Selected Montreal	Vaughan.	
Anne Arundel	" Baltimore Market	J. Balgiano & Son.	
Miller's Cream	Sill's Hybrid × Cassaba	J. J. H. Gregory & Sons.	1885
Missouri	Accidental Sport	D. Landreth & Sons	1892
Extra Early Roof	" "	" "	1878
Cosmopolitan	American × European (?)	D. M. Ferry & Co.	
Green Fleshed Osage	Selection of Grand View	Johnson & Stokes.	
McCotter's Pride	Sport of Peerless	Ferry.	
Long Island Beauty	Selected Jenny Lind	J. M. Thorburn & Co.	1893
Osage Gem	Osage × Netted Gem	Vaughan.	
Defender	Sport of Paul Rose	Ferry.	
Newport	Selected Jenny Lind	Henderson.	
Jersey Belle	" " "	Johnson & Stokes.	
Paul Rose	" Osage	Vaughan.	
Grand View	" Emerald Gem	"	
Carmes	Imported from Syria	Henderson.	
Oklahoma	Rocky Ford × Hackensack (?)	J. W. Tetrick & Son	1902
Khiva Winter	Foreign	F. Barteldes & Co.	
Large California Nutmeg	Selected Runyon	Cox Seed Co.	

Other varieties are largely chance seedlings or crosses. Doubtless much more definite information will be available later.

The following abstracts that I have received through correspondence may also prove of interest in showing muskmelon development:

BANQUET, GREEN-FLESHED OSAGE, DELMONICO AND A MIXED FLESH UNNAMED VARIETY.

We have sold three varieties of muskmelons to the houses that first catalogued them. The Banquet muskmelon was found by us in the hands of a gardener, who could give us no information about it save that he had grown it for a good many years and had first secured his seed from some other farmer or gardener. We could not trace the matter back, and cannot say where or when the variety first appeared. The Green-fleshed Osage came from a variety called the Grand View (which was nothing more or less than an impure strain of Emerald Gem). While inspecting a crop of the Grand View we found a single vine bearing six well-developed melons of an entirely different type; we picked the melons and next season planted the seed, and neither the first season nor at any time since have we found so much as one single impurity in this variety. It came absolutely pure and true from the start, and it is worthy of note that this green-meated variety came from a yellow-meated sort. We had only to continue planting until we had sufficient seed to sell, and then we sold it to Johnson & Stokes, who named it Green-fleshed Osage because of its similarity in size and shape to the Red-meated Osage. The perfected Delmonico was sold by us to Peter Henderson & Co. after some five or six years of selection continued from a 'sport' or impurity found in a field of their Delmonico. This sort kept reverting to the parent type and was very hard to get pure and true to the new form. We have at one time and another saved and continued cultivation for several years on distinct

types of melons (usually sports found in fields of other sorts) to the number of twenty or thirty, only to find at last that the type we were working for could not be perpetuated. We have been working for eight years on a type of muskmelon in which each melon contains both yellow and green meat intermingled, but we have not yet succeeded in getting the variety sufficiently established to sell for introduction. This was produced from a patch in which alternate hills were planted to different varieties, and, curiously enough, is the result of crossing in this manner two *yellow*-meated varieties. We also note that the impurities that we throw out of our yearly trials of this cross are green-meated, and we have not yet found any melons in the cross with wholly yellow meat. We cannot offer any explanation of this fact, nor of what we have said about the Green-fleshed Osage.

CHAUNCEY P. COY & SON.

LONG ISLAND BEAUTY MUSKMELON.

A variety of the Hackensack type, originated on Long Island. It is of very superior quality, with green flesh, and densely netted. In shape it resembles the Hackensack, but is slightly more ribbed, and is decidedly the most attractive looking melon we have ever seen. Besides being the most beautiful, it is also the earliest of all. Out of a collection including every standard variety, it produced the first ripe melon of the season. This gives it especial value for market gardeners. J. M. THORBURN & CO.

MILLER CREAM MELON.

This melon was originated by John D. Miller, of Elmira, N. Y., in the year 1878, it being doubtless the result of cross between Sill Hybrid and Cassaba. It has a very thick flesh, the seed cavity being very small, is of a salmon color, and melting in quality. The rind is thin and of a green color. The vine itself is a strong grower and is very productive, the ground being covered with fruit. This was first catalogued by us in the year 1885.

JAS. J. H. GREGORY & SON.

OSAGE AND OSAGE GEM.

The Osage we believe to be an improved selection from Miller's Cream which was introduced by J. J. H. Gregory, of Marblehead, Mass., who can tell you more about its origin.

The Osage Gem is a cross between the Osage and Netted Gem, and was very carefully made by one of our Michigan growers. The Paul Rose was bred by Mr. Rose, being a carefully selected type of small, solid, round, thick meated Osage, and has a little more netting than the type of Osage which we use. VAUGHAN'S SEED STORE.

FLAVOR.

I have never given any attention to muskmelons except in attempting to cross melons by cucumbers, or more particularly to influence the flavor by the application of cucumber pollen. In this I was never successful.

(Prof.) FRED W. CARD.

VINE PEACH AND GARDEN LEMON.

We are the introducers of the Vine Peach and Garden Lemon. We do not think, however, that they would really belong to the muskmelon family, as they are not usually eaten in the natural condition, but are preserved or pickled. We have never been able to get them perfectly pure. There are always some fruits which are large in size and appear to be midway between cucumber and muskmelon, and we are inclined to think that this vegetable is from a hybrid or cross between the muskmelon and cucumber, but we do not

know where they originated. The Vine Peach we found mentioned in the Chicago Inter Ocean about fifteen years ago, and we wrote to the lady who mentioned this new vegetable and obtained all of her stock of seed. They have since then been renamed Vegetable Orange or Mango Melon by some of the Eastern seedsmen. The Garden Lemon was sent us by one of our Southern customers, who had no name for it, but simply thought that we would be interested in it. It is not really as desirable as the Vine Peach.

IOWA SEED CO.

EDEN.

I introduced the winter melon named Eden, and also have brought into prominence the famous Russian winter muskmelon, named Khiva (Agri. Dept., No. 114), and the N. E. Hansen muskmelon (Dept., No. 116). My business is strictly with winter melons, and I am the only one that has made a commercial success in that line. Have done quite a business with the U. S. Department of Agriculture in testing, and have sold them hundreds of dollars' worth of seeds. J. F. BROWN, Utah.

CALIFORNIA SEED FROM COLORADO.

We get all our canteloupe seed from Rocky Ford, Col.

A. SARBOUGH,
Secretary Coachella Valley Producers' Association, California.

IDEAL.

We have introduced only one muskmelon—our Ideal. It was originated by Prof. Price, of the Texas Agricultural and Mechanical College, College Station, Tex., and he sold us the entire interest in it. It is interesting that we had already named it the Ideal, when we received a letter from Prof. Price suggesting the same name. It is very productive, very sweet, and of better flavor than any melon with which we are acquainted. The flesh is a rich yellow, but occasionally we still find one with green flesh, as one of its parents was green-fleshed. GEO. TAIT & SONS.

NEW ORLEANS.

Regarding the New Orleans Market Muskmelon, would say that it has been 'the only muskmelon' that gave satisfaction in the immediate vicinity of New Orleans. Northern varieties do not succeed so well; they have no taste the first year, and must be acclimated to be of any market value. Even the New Orleans Market Muskmelon does not produce the same sweet melon if the seed was grown North. Our truckers are, therefore, very anxious to procure this seed genuine and of Southern production.

The melon grows to a large size, larger than the large Hackensack; is deeply scalloped, and very roughly netted. Flesh, pale green to yellow, very deep, juicy, exceedingly sweet, and of a delicious flavor.

C. W. EICHLING.

LARGE CALIFORNIA NUTMEG.

The variety of melon called "Large California Nutmeg" is a variety that was grown for many years by a melon grower on the Sacramento River, and was formerly known as the 'Runyon Melon.' We never heard its history or origin. It is the best large shipping cantaloupe that we know of, and we cannot find anything in the East to correspond with it. It is a very large oblong melon, solid, of a nutmeg character, and of a very fine flavor. It is considered the best late shipping melon in California, and commands the highest price.

We have taken hold of this melon for the last two years, as it was partially run out, and have selected the seed so as to bring up the quality.

COX SEED CO., California.

ON GRAPE HYBRIDS

By N. B. White, West Norwood, Mass.

The chief end of every living thing is to perpetuate itself, to reproduce itself, and from the standpoint of a naturalist the most perfect specimens of animals and plants are found in their natural wild state, as they have come down to us under the law of the survival of the fittest, and that the "razor-backed hog" and the wild, seedy and skinny fruits are the most perfect specimens of their kind. But from the standpoint of the epicure, the greater amount of meat the animal has, and the greater amount of pulp the fruit has, the nearer perfection they are in his estimation, and to produce more meat and more pulp the agriculturist and the horticulturist are devoting their utmost energy, and the future results of this energy, as regards fruits, and more especially the grape, it is my purpose here to consider.

The changed environments and the high culture tend to defeat the natural tendency and aim of the vine when in its natural state and condition. Fewer and smaller seeds and better quality are the chief aim of the experimenter, and he rejoices over this distortion of the vine, as the insect rejoices over the excrescence produced by itself having punctured the leaf. This distortion, produced by changed environments, high culture, hybridizing and crossing, tends to produce sterility and to destroy that life principle stored in the seed, as is the case with the potato that produces no seed in Massachusetts. But a cross of the Wild Rocky Mountain upon the Early Rose in Minnesota and the seeds planted in Massachusetts produced seed balls, and fine, large tubers the second year.

The tomato, when introduced some fifty years ago, had many seeds and but little pulp, with a large empty space in the seed cavity, but now the tomato is solid and has but few seeds, and I predict that fifty years hence the best tomatoes will be propagated by cuttings or slips. This same tendency to sterility is observable among grapes. We have a number of seedless grapes, and the writer recently produced another. Most blossoms of hybrids have recurved stamens and fail to properly pollenate the stigma. When using staminate vines as the male parent we get a large majority of staminate vines. The writer once pollenated Black Hamburg with staminate hybrid of vulpina and labrusca and eight vines, and all but one were staminate. As such results are liable to occur, it may be a question as to the expediency of using a staminate vine as a pollenater. Another case, where pollen from a hermaphrodite blossom was used: Seven plants were obtained. Six were fertile, one was staminate, and five of them worthy of cultivation. Just what the difference may be as regards vigor, healthfulness and quality between using pollen from a staminate blossom or a hermaphrodite blossom is yet to be deter-

mined, but the general impression is in favor of the staminate blossom as a pollenizer.

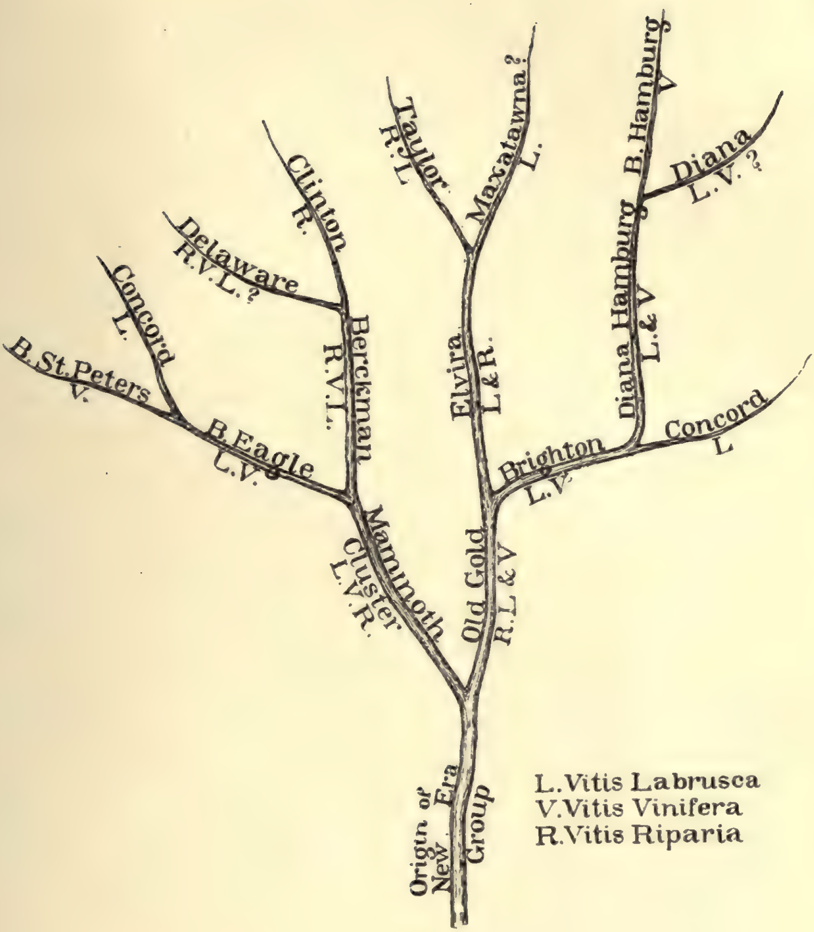
The tendency to sterility from the changed conditions is mentioned here to induce experimenters to secure, before it is too late, specimens of all of our wild native species of grapes, that they may be preserved and be used in the future to rejuvenate the declining vigor of the cultivated vines.

In my experiments in hybridizing and crossing, I find that prepotency and reversion play a very important part, and the prepotency of Vitis vulpina seems to exceed that of all others. It shows itself strongly where there is but a fraction of its blood combined, but shows less strength where Vitis Lincecumii is a part of the combination. Reversion is a subtle element in the production of crosses and hybrids, but is less likely to show itself in the first generation than later on. It is freaky and unstable. There is sometimes a reversion to one parent and sometimes to the other, and the best grape that I have produced is where Vitis vulpina, Vitis labrusca and Vitis vinifera were combined. Vitis vulpina and Vitis labrusca controlled the vine and Vitis vinifera the fruit. The Vitis vinifera used was the Black Hamburg, and the hybrid is of the same color and size as the Black Hamburg and has a decided vinifera quality, ripens early and is hardy. I am fully convinced that our best table grapes will always be produced from the three above mentioned species.

As some grapes do much better on roots of other vines, the writer is now growing graft stocks for the purpose of testing all new hybrids on. These stocks are from a very vigorous, hardy vine that will take the graft readily.

If the perfect blossom and fertility of the vinifera grapes are brought forward to disprove the tendency to sterility of crosses and hybrids, I would say that there is no evidence that the vinifera grapes have been brought up to the present state of perfection by crossing or hybridizing. On the contrary, it is probable that they have been improved by long years of selection.

I am now devoting my attention to the establishing of a basis for future viticulture by combining such species as are most likely to furnish desired qualities when the seeds of these combinations are planted. The accompanying diagram will show a combination having in it V. labrusca, V. vulpina and V. vinifera for table grapes; another has in its combination V. labrusca, V. vulpina, V. Lincecumii, V. rupestris and V. vinifera. The former combination should furnish large bunches and berries, and the latter should furnish large clusters and small barries. Such has been my experience with these combinations thus far.


B. Hamburg
V
Diana
L.V.?
Taylor
R.L
Maxatawna?
L.
Clinton
R.
Delaware
R.V.L.?
Concord
L.
B. St. Peters
V.
Berckman
R.V.L.
Elvira
L & R.
Diana Hamburg
L & V
B. Eagle
L.V.
Brighton
L.V.
Concord
L
Mammoth
Cluster
L.V.R.
Old Gold
R.L & V
Origin of
New Era
Group
L. Vitis Labrusca
V. Vitis Vinifera
R. Vitis Riparia

PRACTICAL POINTS FROM THE BREEDING OF STRAWBERRIES AND BUSH-FRUITS

By Fred W. Card, Horticulturist, State Experiment Station, Kingston, R. I.

The work of the plant breeder never lacks interest. He is led on by many deep-thought theories and more delightful possibilities. Limitless improvement seems to be within his reach, but in practice he meets with many difficulties. Nature does not lightly grant her favors. She may coyly keep them just beyond the grasp of the would-be possessor while steadily luring him onward.

In the breeding of strawberries and bush-fruits some difficulties have been met. To call attention to them is the purpose of this paper, rather than to record the results accomplished. In 1899 strawberry plants of a number of different varieties were fruited in hills. Careful records were kept of the yield of each plant. The berries from each picking were counted and weighed, observations being made upon the color, firmness and other qualities of the plant and fruit. Selecting from these records two varieties, Bismarck shows an average yield of 106.4 grams per plant; Glen Mary shows an average yield of 460.1 grams per plant. Practical selection often stops at this point. The Bismarck would be discarded, the Glen Mary planted; or perhaps the selection may go one step farther, a step indicated by the following record. Parker Earle produced berries weighing on the average 2.81 grams each. Hunn produced berries weighing 5.86 grams on the average. Here, then, is another factor which may influence the grower in his selection of variety; but let us look further. Plant No. 19 of the Bismarck variety produced only 15.3 grams of fruit, weighing on the average 2.18 grams each. Plant No. 14 of the same variety produced 233.6 grams, the average weight being 5.99 grams each. Plant No. 7 of the Glen Mary variety produced 286.6 grams of fruit, weighing 4.62 grams each on the average. Glen Mary No. 12 produced 756.3 grams, the average weight being 6.10 grams. Parker Earle produced the smallest fruit of any of the varieties grown, the average weight being 2.88 grams, but the average weight of fruit from plant No. 18 was 1.29, while the average weight from plant No. 6 was 4.04. Hunn produced the largest fruit, the average weight being 5.86, but the average weight from plant No. 9 was 4.17, while that from plant No. 13 was 7.5 grams.

In 1900 five plants were chosen in a field of Kansas raspberries as the fruit approached ripening. The plants chosen were those which appeared to be among the best plants so far as the eye could determine, yield, size and general character of plant being considered. Of these five, plant No. 3 produced 648 grams of fruit, the average weight being 1.13 grams each. Plant

No. 5 produced 1,130 grams, the average weight being 1.33 grams. In other words, plant No. 5 showed an increase of 75% in yield over plant No. 3 and an increase of 18% in size. In 1900 these same plants produced as follows: Plant No. 3, 1,104.4 grams of fruit, averaging 0.96 grams each; plant No. 5, 2,393.5 grams, averaging 1.05 grams each. Plant No. 5 showed, therefore, in this year an increase of 117% in yield and 9% in size over plant No. 3. It should be remembered that these are comparisons between five of what appeared to be the best plants at the time of selection. The second year as fruiting approached five poor plants were also selected, choosing not the smallest plants which could be found, but those of apparently the poorest type and generally small. The average yield of these five plants, not to take the poorest one, was 154.3 grams, the average weight of berry being .74 grams. Comparing plant No. 5 with the average of these five poor plants shows that its increase in yield over their average yield was 1457%, the increase in size of fruit being 42%.

These figures are introduced to emphasize the fact of individuality in plants. There is a radical difference in behavior of different plants of the same variety in the same field. What contributes to this individuality? Probably many things. The environment, though apparentlv the same, may in fact differ greatly. Perhaps in distributing the fertilizer a larger amount may have fallen near one plant. Soil moisture is variable; perhaps the conditions in one place may be more favorable. Perhaps one plant may have suffered from an insect attack or from some mechanical accident which no longer shows. Perhaps the vigor of the plants may have differed when set, owing to one plant having been grown under better conditions than another. Even under the best of conditions of field culture such possibilities always exist. Yet when ample allowance has been made for them all, there still remains the fact that the inherent vigor of the individuals must differ. How great this difference is, is a point needing further investigation. Probably it is often underestimated; perhaps in our zeal for plant breeding we overestimate it. To learn something of its importance in connection with these strawberries has been one of the points under investigation, and it is in connection with this trial that some of the difficulties have been met.

The most productive plant from eight of the most promising varieties was chosen from which to continue the selection, the aim being to constantly select young plants from the parent giving the largest yield. The first obstacle met was found to be the loss of vigor entailed in the production of a heavy crop. In one or two cases the plant producing the largest yield was so thoroughly exhausted thereby that young plants could not be obtained with which to carry on the selection. In others, they were evidently lacking in vigor. The first move, then, was to provide against this difficulty by selecting from all the plants of a variety under trial, before fruiting, a sufficient number of young plants with which to carry on the selection, then all are discarded except those from the plant which at fruiting time proves most productive. This adds greatly to the number which must be cared for at any given time and complicates the work.

A second qnestion has since arisen. It is always somewhat troublesome to keep plants in hills, and particularly so when, as in this case, a few runners

must first be permitted to grow a sufficient number of plants with which to continue the selection. Does the yield from a single hill represent the actual value of a variety, as commonly grown, or would a better representation be obtained by giving the plant when first set plenty of room and allowing it to make plants at will, forming an individual colony made up of the mother plant and its offspring, then using the yield from this colony as a basis of estimate? This comes nearer what the plant must do in actual practice, since nearly all strawberries are grown commercially in matted rows. In future this is to be our plan of selection.

The difficulty of securing uniformity of conditions, as already suggested, is an ever present one, and demands the utmost care at every turn. Not only do conditions of soil and surroundings differ, but conditions of care. Probably no two men would set a plant in the same way, and perhaps no two would hoe it in the same manner. We are still a long way from absolute accuracy in such matters.

Some work has been under way in crossing. Among bush-fruits my first experience was at Cornell University, where a number of crosses were made between different varieties and species of raspberries and blackberries. The general results showed nothing of value as a result of violent crosses; in fact, nothing worth perpetuating resulted from any of the work, but the most promising offspring came from crosses between parents most closely related. Perhaps a few brief notes from these taken from "Bush-Fruits" may have a bearing upon recent discussions. Of five plants of Gregg × Shaffer in fruit, four resembled Gregg in character of plant and one resembled Shaffer. In character of cluster three approached Shaffer and two resembled Gregg. The fruit in one case closely approached Shaffer, while in others it was intermediate or nearer the Gregg. Of five plants of Fontenay × Cuthbert, three resembled the female parent in character of growth, one the male, and one was intermediate. Thirty-one plants of Shaffer × Cuthbert were grown. The majority resembled the male plant in character of plant and the method of propagation. Some produced typical red raspberry fruits, while the fruit of others was dark, resembling Shaffer. Three plants of Ada × Cuthbert all resembled the male parent in character of cane. In Rhode Island not enough plants have as yet fruited from which to draw conclusions.

A number of strawberry seedlings have been fruiting during 1901 and 1902. In general it has been noticed that there is a great similarity between the seedlings of the same cross, though there are marked exceptions to this rule. In some cases seedlings are so similar that they would readily be considered a single variety. A few observations were made upon the sex of offspring from perfect and imperfect parents. Although not observed fully, the following results will show something of the tendencies:

Bubach (I) × Wilson (P). Seedlings, perfect 5, imperfect 8.

Bubach (I) × Wm. Belt (P). Seedlings, perfect 8, imperfect 6.

Crescent (I) × Glen Mary (P). Seedlings, perfect 22, imperfect 28, weakly perfect 4. In this case the perfect plants did not as a rule bear very many stamens.

Glen Mary (P) × Wm. Belt (P), perfect 47, imperfect 1. Some were weak pollen bearers, being nearly imperfect.

Hunn (I) × Ideal (P), perfect 21, imperfect 9.

McKinley (P) × Ridgeway (P), perfect 15, imperfect none.

These observations seem to show a fairly equal division in influence of the two parents in the matter of sex.

A few general notes upon seedlings of different crosses may be of interest.

Wm. Belt × Wild strawberry. Plants of this cross all possess the characteristics of the wild plant, making an abundance of runners, with comparatively small foliage. The characteristics of the wild fruit also persist, though with an increase in size and some variation in shape. The deeply set seeds and sharp corrugations, with bright color and a pronounced wild flavor, exist in all. One plant in particular showed a promising increase in size of fruit with a very desirable form.

Hunn × Ideal. Behind this label we always expect a sturdy, low-growing plant with dark green foliage, bearing a short, roundish, dark red berry, with dark red flesh, rather sprightly in flavor, and ripening late. An occasional variation both in color of fruit and foliage occurs, but in the main the type is well marked.

Crescent × Glen Mary follows Crescent closely in general characteristics, the fruits usually being even smaller.

Glen Mary × Ridgeway has produced a strong, vigorous plant, rather light in color, fruit rather long and sometimes flattened, light colored and sweet.

McKinley × Ridgeway produces light colored fruit, long and flattened at the apex; sweet, but rather lacking in sprightliness. In marked contrast to this general type, one plant produced very short, round fruit, almost as round as an apple, but possessing other general characeristics of the cross.

The plants show considerable difference in their ability to resist rust. The Wm. Belt × Wild seedlings are particularly susceptible. Among other varieties, here and there a plant would stand out in marked contrast with the rest of its family.

No second generation plants have yet been fruited, so that it is impossible to speak of variations in the second generation.

The chief difficulty to be overcome in obtaining desirable varieties by crossing lies in the large number of characters desired. These embrace vigor of plant and leaf, plant-making ability, resistance to rust, hardiness, productiveness, size of fruit, color, shape, including the way in which the hull is set, firmness, flavor, season of ripening, etc. Whether Mendel's law is a fact or a fad, it is difficult to combine all these characters which are desirable in one berry. There is abundant room for advance. Our best berries are still deficient in some points.

HYBRIDS AND DISEASES

By L. H. Pammel, Botanist, State College, Ames, Iowa.

In studying the diseases of plants the writer has been interested in observing the comparative immunity of certain plants and the abundance of fungus diseases on others. Not only are there striking differences in plants that apparently belong to the same species, but these differences are noticeable in certain hybrid forms. In the "Life and Letters of Charles Darwin" is an account of experiments made in England to breed a variety of potatoes that would be comparatively free from the potato rot fungus. It has been shown by Dr. Erwin F. Smith and others who have paid particular attention to the plant diseases that it is possible to breed certain varieties of plants that will be more resistant to the attacks of certain fungi. It seems to me that the attention of plant breeders should especially be called to the importance of this subject from an economic standpoint. In order to overcome the difficulties careful studies must first be made of the characteristics of the plants that are used for cross breeding purposes. I will cite a few illustrations of how the diseases have manifested themselves in the progeny by not paying sufficient attention to the question of eliminating bad qualities that exist in the parents. Where the hybrid or cross comes by chance this question cannot be eliminated, but where hybrids are produced as a result of careful hand pollination these undesirable points should be eliminated as far as possible.

Raspberry Hybrids and Diseases. A few years ago I obtained what was said to be a chance seedling of the red raspberry. I was told that it was a highly desirable plant, much hardier than the red raspberry, and free from diseases. A considerable number were planted in my garden. After a study of the plants I became convinced that this chance seedling was a hybrid between *Rubus strigosus* and *Rubus occidentalis.* In fruit it had partially the characteristics of both. The leaf was nearly intermediate between the two species, except it partook more of the character of the *R. strigosus* than the *R. occidentalis.* The stem was intermediate in color between the two species, and its method of propagation was that of the black cap raspberry. For some years the anthracnose (*Gloeospórium venetum*) has been common in the vicinity of Ames upon several varieties of the black cap raspberry. Not far from this patch of black cap raspberries the new seedling raspberry was grown. For several years the plants were seemingly very thrifty, bore abundantly, and there was no evidence of disease. But during the last three years this parasite has been nearly as destructive to this hybrid seedling as to the black cap raspberry. I made a careful search for this fungus upon the red raspberries growing in the same vicinity in my garden, but without, however, finding any trace of this fungus, nor have I ever col-

lected it upon this host in this State. The fact that this fungus has occurred so abundantly upon this chance seedling, coupled with the other characters, leaves no doubt in my mind as to thehybrid origin of the seeding. (Since writing the above the writer has found the fungus in the red raspberry.)

PLUM HYBRIDS AND DISEASES. For some years plum scab (*Cladosporium carpophilum*) has been abundant upon different varieties of the *Prunus Americana.* It is true that some years it is much more abundant than others, some years being so common as to seriously affect the market value of plums. The writer has never seen this fungus upon the European *Prunus domestica* or the Japan plum. Some varieties of the American plum are much more seriously affected than others. I was, therefore, very much interested in finding that certain hybrid varieties of the *P. Americana* crossed with the Japan plum had some of the plum scab. Of the seedlings obtained from the same experiments there are some varieties of the hybrids that are exempt, but in one variety the disease occurred successively for several years.

THE WILD CRAB. In our State the wild crab is seriously affected with several fungus diseases, notably the cedar apple fungus (*Roestelia pyrata*) and the apple scab (*Fusicladium dendriticum*). Some varieties of *Pyrus malus* are also commonly affected with the apple scab, but never, so far as I know, with the cedar apple fungus. The Soulard crab, which is generally now regarded as a hybrid between the *Pyrus malus* and the *Pyrus Iowensis,* is usually free from the Roestelia, though occasionally I have seen a few diseased leaves when in proximity to the red cedar. It is also slightly affected with the apple scab fungus. The Mercer County crab, which by some writers has been regarded as a good form of *Pyrus Iowensis,* and by others has been regarded as a hybrid, and I am inclined to this latter view. It is very different with respect to its diseases from the Soulard crab. I have had a tree of this variety under observation for eleven years, and he has watched from year to year in a general way the amount of fungus diseases affecting the fruit and leaves. In most seasons it is nothing unusual to find that a large number of the leaves are affected with the *Roestelia pyrata.* During all this time the writer has never seen this fungus upon the fruit or branches, and yet it has been very common upon both fruit and branches upon our wild crab. Apple scab is rather rare on the fruit, only occurring upon the leaves.*

GOOSEBERRIES. The native wild goosberries in this State, both the *Ribes gracile* and *R. Cynosbati,* are seriously affected with the gooseberry rust (*Accidium grossulariew*). Taking the Champion as a type of the cultivated species of the European *Ribes grossulariæ* is not affected, at least not seriously, with this fungus. A hybrid of this species and the *R. gracile* had a few diseased leaves and fruit the past spring, but generally speaking it is quite free from this fungus. In regard to the *Septoria Ribis* occurring upon both the wild species and the cultivated, I may say that the past season has been extremely detrimental to the gooseberries, the leaves falling off prematurely. The hybrid has retained its leaves much longer than the cultivated gooseberry, indicating that the hybrid is superior in the quality of resistance to plant diseases as compared with the cultivated forms.

In order that we may look up this matter a little more, I hope that anyone interested will give to me such information as he may have on this subject, and shall be greatly obliged if I could receive specimens.

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*Hume and Craig. Native Crab Apples and Their Cultivated Varieties. Proc. Iowa Acad. Sci. 7: 123.

HYBRIDISM VS. SELECTION

By F. W. Burbidge, Curator, Trinity College Botanical Gardens, Dublin, Ireland

Hybrids are of two descriptions, those produced naturally or spontaneously in the wilds, and those raised artificially in the garden, but there is no real distinction between them. We are told that a hybrid is the offspring of two species, one or both of which at times may be either pollen or seed parent. But the day of a rigid belief in "pure species" of plants is past, and to say any plant is a species, simply means the expression of one's own, or somebody else's, judgment or opinion. A species is merely some botanist's decision, and not nature's decision, for how can nature be decided, seeing that evolution is continually going on? A species often includes an enormous number of individual plants varying more or less among themselves, and which come more or less true from seed. A species is, in fact, often a very variable quantity, and its capacity for variation is absolutely unknown, except as it is experimented upon in the garden or elsewhere. Our ignorance of the natural history of plants is profound. Two so-called species grow in the same soil and situation, and belong to the same natural group or order, and yet while one is a useful food plant, the other is a virulent poison to men or other animals.

Again, two plants called species, may grow on the Andes or Himalayas side by side, and yet, when brought to American or European gardens, the one may be quite hardy, while the other dies unless sheltered in an artificial temperature. Why plants thus vary in their secretions or products, and in hardihood, no one knows; that they do so is a fact patent to the most ordinary observer, and these problems await solution from the biologists of the future.

Again, two related species will, when hybridized together, sometimes produce fertile offspring, and in other cases barren ones. Sometimes species A will fertilize species B, but species B will not fertilize species A, but why, no one knows. In other cases, two or more species will be reciprocally fertile, but why this is so, neither physicist nor physiologist can say. any more than they can tell us why one plant secretes or makes sugar and another starch, and others wine and oil—nutritious food—healing medicines or deadly poisons. These so far are the secrets of nature's great laboratory.

But let us come to the hybrid. The whole history of hybrids is obscure, and in many cases the so-called records are most unreliable. In the case of so-called spontaneous or wild hybrids what has happened is this: The armchair botanist, knowing nothing of the circumstances of their origin or native environment, has simply named and described them as pure species! Now and then, as in the case of some orchids, a guess as to their parentage

has been made by collectors abroad and even rarely by botanists at home, and we have instances where orchids and other wild hybrids have been made over again by fertilizing the parent species in our hothouses here at home. Nevertheless there are thousands of wild hybrids lying obscured under Latin specific names in our books and herbaria throughout Europe and America to-day. As to garden hybrids, in the beginning of the past century it was thought impious to molest nature, and the early botanists and students of plant hybridism were pretty much in the position of the surgical vivisectionists who, rightly or wrongly, experiment on living animals to-day. In a word, they worked in secret and scarcely wished or dared to tell the truth! It is curious to observe that while physical unions such as inarching or inoculation, budding and grafting, were looked upon as quite respectable and clever, the physiological unions by cross pollination were universally tabooed, and in Northern Europe more especially. I say in Northern Europe, because in South Europe, North Africa and the East, the necessity for fertilizing the fig tree and the diœcious date palm artificially had been carried out from very early times.

In English gardens hybrids have been reared designedly for a period of well night two hundred years. The first of garden hybrids recorded, in England was Fairchild's Mule Pink, said to have been raised at Hoxton, near London, before 1719, between Dianthus caryophyllus, and D. barbatus; that is to say, between the Carnation and the Sweet William. This and many other early hybrids were called "mules" from an erroneous belief that, like the horse and ass hybrid so called, all vegetable hybrids were likewise sterile. The early history of garden hybrids has been obscured by the secretive character of the early experimenters and the jealousy they felt of each other.* Also by a more or less superstitious fear of revealing or recording what at the time was regarded as an irreligious or sacrilegious interference with nature. There were also later on trade jealousies, and hybrids were either said to have come from abroad, whence their parents had come before them, or their origin was disguised and concealed under specific Latin names. One remarkable instance of these latter tactics being adopted on a large scale occurred when Messrs. Rollison, of Tooting, and other growers of Cape heaths, at a time when they were nearly as popular as orchids and begonias are to-day, reared numerous hybrids and seedlings all of which were credited to the Cape of Good Hope and duly christened with Latin names.

Another potent source of error as to garden hybrids is due to the fact that, fertilization having been effected by wind or insects, the seedlings that varied were assumed to have been hybrids. In a word, the seeming intermediates were assigned the most probable or obvious parentage without any real proof.

This brings me to the point of this paper, viz.: That the parentage of an enormous quantity of hybrids depends on mere "guesses at truth" and not on any accurate records whatever. It is difficult to estimate the dire results of this practice as a source of error, because intermediates are often

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*See Gardeners' Chronicle, 1890, July 26, page 103.

produced in gardens by ordinary seminal variation, and without any hybridizing operation whatever.

We are apt to attribute too much to hybridism as a motive power in producing variations, and even in the blending of characters among cultivated plants.

Before we can be sure of what hybridism effects, we must know exactly how far the parent species themselves can vary as self-pollinated. It is self-evident that some species which so far as we know have never been hybridized can and do vary infinitely as cultivated. In a word, cultivation and the inter-crossing of varieties yield results at times almost, even if not quite, as great as does hybridism. The Chinese Primula, Cyclamen latifolium, the Gloxinia or Sinningia, many root crops and cultivated vegetables, which so far as we know have never been hybridized, yet vary as much as those plants which have been so originated. We have only to look at the immense variations in apples, pears and other domestic fruits in order to recognize the great central fact that cultivation—the crossing of seminal varieties—and human selection are quite as potent as, or even more so than, hybridism alone. Of course, hybridism as confined to so-called species and the cross breeding of varieties differ only in degree, both being sexual and physiological processes. Many of our type species even as wild plants are extremely variable from seed, just as many seminal garden varieties come practically true from seed! As a matter of fact the distinction between species and varieties is an arbitrary one, but it is for the present a convenience to keep up the nominal distinction. Some day it will be recognized universally that garden species artificially reared are quite as distinct botanically and often a great deal more useful than the native or wild ones. Hybridism often, it is true, gives us a splendid starting place—a spring board, or a new field of variation as it were, but that field must be further improved by cross breeding and selection or the highest and best of practical results are lost, or in any case not actually realized.

We must clearly grasp the fact that the three great factors in the making of plant products more useful or suitable to our daily wants are cultivation, the cross breeding of varieties, and a careful selection of the most suitable or desirable seedling kinds. Even cultivation and selection alone from wild plants, as in the carrot and parsnip of Vilmorin's and Buckland's experiments, will work wonderful transformations in only a few generations.

The fourth factor, viz., hybridism, is potent in the origination of new races, as illustrated in the Tuberous rooted Begonias, the large flowered Cannas, the Gladiolus, Marliac's colored Water Lilies, and many other things; but the initial gain still depends on the other three factors for its full development.

I doubt very much whether the newly discovered "Mendel's law" will be of much practical service to the ordinary hybridist, or whether it will lead to a more precise and exact system of working among hybridists or breeders in the future. In conclusion, I may put forth the following suggestions to those who live in this country, fertile as it is in experimental stations and gardens of all kinds. As a rule, I know the best practical results in hybridizing and cross breeding have been obtained by going direct to the point,

but the losses have also been very great under this plan. The scientific way is to do one thing at a time, and work from the simple to the complex. In this way I would suggest that ten or a dozen suitable species should be selected for experimentation.

One plot of individuals should be well cultivated and self-fertilized, their seeds being again sown so as to get at the simple results of good cultivation and selection alone. The plants in plot 2, under the same conditions, should be carefully hybridized, reciprocally if possible, and the seeds of these should be again sown and well grown. Selection might be made in both cases, the object in view being to decide whether the simple selection of self-fertilized seedlings does not play a larger and hybridization alone a smaller part in the evolution of garden plants than is at present believed to be the case. The experiments could then be continued with the same material, so as to determine the importance of the part played by the cross breeding of the selected varieties in both cases. As it is, we are in "going direct" working with unknown factors. We must first of all find out how our parent species behave under, 1, culture; 2, selection; 3, cross breeding, and 4, hybridism, instead of hybridizing first and trusting to chance for our results. When we see the wonderful results attained among live stock, cattle and poultry, as well as among fruits, vegetables and flowers, by cross fertilization and selection, we may realize that after all hybridism is not everything in the evolution of the most useful animals and plants of both the farm and the garden.

NOTES ON CALIFORNIA PLANT BREEDING

By E. J. Wickson, University of California, Berkeley, Cal.

Plant breeding has been pursued in California ever since the establishment of the missions by the Spanish padres. The first of these establishments was made at San Diego in 1769, and here the first cultivated fruit was grown. Gardens surrounded also the missions established later as the padres proceeded northward through the coast region of the State. Many kinds of fruit were grown, and quite marked differences in the varieties of the same fruit were noted by visitors to these missions before the date of American occupation, and many of the fruits survived after that date. While the "mission grape" and the "mission fig" were the same at all the missions and indicate continuous propagation by cuttings, the mission olive has local variations which have never been accounted for. The deciduous fruits varied greatly and seem to indicate selection from seedlings. There is no evidence that the padres practiced budding or grafting, and there are some reasons for thinking that they relied upon growth from seed and secured better varieties here and there by selection, although they developed nothing by the process equal to the varieties known to Europeans and Americans at the middle of the last century.

Very soon after the American occupation and the announcement of gold discovery, a sharp interest arose in new varieties of fruits upon the widely prevalent idea that such varieties would be better adapted to local soils and climates than the popular sorts of the humid regions of America and Europe. There were thousands of seedlings to select from, because seeds and pits were easily brought along the various routes followed by the pioneers, while the shipment of nursery stock was very difficult and expensive. The first fruits grown in the State by Americans were counted worth as much for seed as for pulp, so sharp was the demand for the multiplication of trees. Many very satisfactory seedlings were fruited, some of which have ever since maintained their places in the fruit lists of the State. When the introduction of grafted trees from all parts of the world began by enthusiastic horticulturists who came from all civilized countries to the new El Dorado, there became available many new elements of parentage. It may be doubted whether in any part of the world so many varieties came to fruiting at the same time as were to be found in Central California. The growth of seedlings continues still, in the belief that the wonderfully favorable conditions for growth would produce horticultural wonders in size, beauty and quality. Of course, not all such anticipations were realized. Selection of seedlings began to be pursued upon a rather more rational ground, namely, to secure particular adaptations to local needs in season of ripening, in suitability for preservation and transporta-

tion, and in many other characters, which were seen to be locally desirable. How widely and how definitely also this selection of seedlings was pursued and what satisfactory results were secured are shown in considerable detail in a paper which the writer contributed to the Proceedings of the American Pomological Society, Session of 1895. This watching for wonders in chance seedlings is still a passion of the California fruit grower, and desirable acquisitions are still being disclosed, although each year brings new casualties to the fruit lists. There are probably not one-tenth as many varieties of all kinds of fruits, both citrus and deciduous, now growing in California as there were twenty years ago. Although many new varieties have been secured both by selection of seedlings and by the higher arts of plant breeding, ten times as many have been dropped from the lists, not because they were failures as fruits, but because they did not meet the very sharp requirements of commercial fruit growing as now pursued in California.

During the last decade plant breeding in California has rapidly widened in scope and advanced in aim and method. Though our most distinguished plant breeder, Mr. Luther Burbank, began his California life and effort as early as 1875, it was not until some years later that results began to appear and the people to understand his lofty purposes and wonderful achievements. This disclosure of a horticultural prophet of the highest type has naturally stimulated plant breeding and led to higher arts and greater ambitions, and the results secured by others than Mr. Burbank are becoming notable. It has seemed to me that even a rough sketch of what other Californians than Mr. Burbank had achieved, with some mention of their beliefs and methods, would be acceptable, and would indicate that under the favoring conditions in California excellent work was being done by many enthusiastic and devoted plant breeders. This elimination of Mr. Burbank is necessary because his work alone, even cursorily discussed, would occupy more space than this writing should claim; besides, information about him and his work was given by the writer in four issues of the Sunset Magazine, San Francisco (December, 1901, and February, April and June, 1902,), to which the reader is referred.

For the purpose of securing up to date and authentic data from California plant breeders I addressed letters of enquiry to those whom I knew to be engaged in this work. All did not comply with my request for information, and if omissions are noticed they may be due to this fact. The replies to these enquiries follow:

The Loganberry and the Mammoth blackberry are the only plants of any value that I have originated.

In August, 1881, I planted the seed of the common wild blackberry, or dewberry, of California, botanically known as the Rubus ursinus, gathered from plants on one side of which was growing a kind of evergreen blackberry known as the Texas Early, and on the other side of which was growing an old variety of red raspberry. The Texas Early has a growth of cane and leaves similar to the Lawton, although much less vigorous, and in our mild climate is growing winter and summer. It has a small round berry of more acidity than the Lawton and probably of poorer flavor. The raspberry referred to has been growing in this place for the last forty years, and I am unable to ascertain what variety it is, although it is of a type similar to the Red Antwerp. It is not, however, the Red Antwerp as we have been growing it here. From this seed there grew about one hundred plants.

In the summer of 1883 these fruited and there appeared one plant which was undoubtedly a cross between the raspberry and the R. ursinus. The fruit was larger and earlier than the raspberry or any blackberry, except the ursinus, ripening about the middle of May; the appearance of the berry on the surface was something like the raspberry, being less indented and of more even surface than a blackberry; the color a bright glowing red, becoming very dark and finally, when dead ripe, of a dull purplish-red color. The berry has a core like the blackberry and parts from the calyx the same as a blackberry. The leaves of the vine are almost identical with the wild Rubus, being somewhat larger. The canes are also like the wild Rubus, but larger and more vigorous; it has the same small, sharp spines, and, like it, is without adventitious root buds, but multiplies from the stolons or tips and from seed. The fruit, when cooked, has the same rich acidity as the wild Rubus, there being only a suggestion of the taste of the raspberry in the cooked fruit, but in the jelly there is a more decided raspberry flavor. This red berry is universally known here as the Loganberry. It is an enormous grower and bearer, there having been gathered in this city full twenty-five pounds of fruit from one plant in one season. In Southern California it is fast displacing all other blackberries.

The other plants produced at this time, being crosses between the ursinus and the Texas, also developed into an entirely new type of blackberry, most of them of good quality; equally good for canning and jams as the Loganberry or the wild Rubus, and almost as early in ripening.

Since 1881 I have planted a good many seeds of this Rubus ursinus fertilized with the Texas Early. About twelve years ago there appeared among these seedlings—and it is uncertain from what year's planting it came—a remarkable blackberry. The canes are enormous. I have a plant now growing in my grounds which grew one cane or stalk last year, for this year's fruiting, of one hundred and forty-nine feet of fruit wood. This single plant will cover with foliage a wall forty feet long and from six to eight feet wide. The fruit is equally colossal; berries are frequently found two and one-half inches long. The fruit is similar to that of the wild Rubus, being less sharply acid, and when perfectly ripe is sweet and delicious. This berry I have named the "Mammoth." Its fruit is similar to the Loganberry, but less acid. The Mammoth fruits perhaps a couple of weeks later than the Loganberry, and is jet black in color.

The raspberry parent of the Loganberry is, like most raspberries, prone to spread from adventitious rootbuds; the Texas Early is also a perfect nuisance in that respect. The Rubus ursinus has no adventitious rootbuds, but propagates entirely from the tip. And it is a singular fact that, in the thousansd of seedlings of the Loganberry and of the crosses between the Texas and the ursinus and crosses between plants thus crossed, not a single plant has been found that had adventitious rootbuds, but, like the female ursinus parent, all reproduce from the tips or seed. As is well known, the raspberry has a perfect bi-sexual flower. The Rubus ursinus, sexually, is divided into the male and the female. Such a thing as a bisexual flower in the Rubus ursinus is unknown, and it is a characteristic of that plant, growing wild in the woods, while the cane of the male plant is very much smaller and apparently less vigorous than the female, the male ultimately speads in rich soil and completely chokes out the female plants so that in a few years the berry patches become entirely barren, being constituted entirely of male plants. It is very noticeable in cases where the woods have been burned over: for the first few years, the burned district will produce many blackberries; in a few years, however, the productive berries entirely disappear and the male berry takes entire possession.

The flowers of the seedlings which I have grown have been mostly bisexual and very large. I never yet have seen a flower of the Loganberry or of any of its seedlings that was not bi-sexual and perfect. Very many, how-

ever, of the crosses between the ursinus and the Texas are uni-sexual female, but I never yet have seen a flower of any of my seedling plants that was uni-sexual male, like the ursinus produces.

My experience with the hybridizing of the Loganberry and the Mammoth blackberry with each other and other fruits, has resulted in some very interesting, and, in many cases, peculiar horticultural productions. The seedlings of the Loganberry, having been propagated in this State by the thousand, very many of them by myself, have as far as I know, resulted in the reproduction of the Loganberry type only; I never yet have heard of one of these seedlings returning to either a raspberry or a blackberry. The fruit of these seedlings is always the same red color; the same general flavor, and the vines have the same general appearance; but, as in the case of all seedling plants, the fruit of ninety-nine out of a hundred plants is not equal in any respect to the original, in fact they are mostly worthless.

I have made many crosses of the Loganberry with my seedlings of the Texas and the ursinus. No. 1 is a cross of the Logan with a pistillate product of the Texas and ursinus; No. 2 is a cross of the Logan with the wild ursinus; No. 3 is a cross of the Logan with the Mammoth; No. 4 is a cross of a pistillate product of the Texas and the ursinus with the ursinus.

The result of all these crosses has been a most peculiar failure. Not a single one has been prolific, although in most instances the blossoms have both pistils and stamens perfectly developed in the same flower. A few have been pistillate (uni-sexual female). Not a single one has been uni-sexual male. It would be supposed that such flowers would produce fruit, but they are utterly barren. Out of hundreds of plants I have not found a single perfect berry and very few imperfect ones. These hybrids, while perfect and valuable as producers of fruit, and constant in reproducing themselves, seemingly refuse to be a party to any more crosses.

J. H. LOGAN. Santa Cruz, Cal.

While not entirely so, yet most of our work is done by hybridizing. Some of our results are:

Snapdragon Sweet Peas ("Pure White," "Light Pink," "Deep Purple"). —The standard never expands, but closely overlaps the wings, giving the flower a bud-like form. (Introduced, 1901.)

Sweet Pea (Giant-Flowered "Chamberlain").—While all cupid sweet peas have larger sized flowers than the same varieties of the original tall type, this one is truly entitled to the name Giant-Flowered. The extraordinary size of the flower is emphasized by being produced in wonderful abundance upon such dwarf compact plants. Striped rosy pink on white ground. (Introduced, 1900.)

Fall Nasturtium "Croesus."—This is a distinct new climbing Nasturtium. Foliage rich and dark. Flowers are of immense size, rich sulphur yellow. Each petal is splashed or tiped with red. The two upper petals are marked with large peacock-feather markings of deep red. The lower petals are also marked with the peacock eye, large and distinct, but of a soft rose color. This variety has the peculiarity of commencing to flower with the yellow and red markings, and as the growth of the vine continues there will be flowers of rose red, with yellow lines upon them and often four or five distinct flowers will be upon the same vine. (Introduced, 1903.)

Ivy Leaf Nasturtiums.—The original was propagated in Europe—came very untrue to type and of one color. We have succeeded in not only getting a true type but, by crossing, some twenty different colors.

Unique Sweet Pea. "Salvation Lassie."—This is new departure in sweet peas. The standard is unusually wide and well expanded, yet curving so curiously over the wings as to suggest a bonnet. Color a soft shade of deep rose throughout. (Introduced, 1902.)

L. C. ROUTZAHN, Manager,
McClure Seed Company, Arraya Grande, Cal.

I have fruited many seedlings, but have not produced anything superior to what we already had. From 1869 to 1878, when there was no sale for pears and no codling moths, we could pick up free of charge all the pears we wanted in orchards where they had dropped. Many men raised their own pear seedlings. One year I selected 400 out of 60,000 seedlings grown from grafted fruit, worked them high on small limbs of old trees, these nearly all fruited in four and five years, but none proved superior to those varieties we had in cultivation. I have the same experience with plums of which there are two good varieties not yet introduced.

Three years ago we selected out of 120,000 apricot seedlings 500 and budded them high on peach trees, some show very well for bearing, size and ripening. We have also a lot of almonds on trial, worked the same way, high on old trees. JOHN ROCK,

California Nursery Company, Niles, Alameda Co., Cal.

There are at Home Orchard, two apples, one apricot and two peaches that I think have some claims for excellence, all chance seedlings.

One of the apples is a seedling of the Grindstone, somewhat larger—a yellow ground with light and darker red stripes—shape flatish round—both stem and eye cavities deep—stem slender, core very small; seeds plump and dark. Fruit fine grained, moderately juicy, rich, with a slight acid flavor—a good eating apple at Christmas and will keep till May or June. Tree an upright, strong, healthy grower. The original tree grew just within the Fresno grove of "Big Trees" (Sequoia gigantea) and is twenty-five or thirty years old. A few trees have been propagated and sent out as Sequoia.

The other apple originated here—a cross of White Winter Pearmain with G. N. Pippin. It retains the peculiar flavor of the Pearmain with the rich sharp acid of the Pippin. Color a greenish yellow, size medium, shape flatish oblate, with prominent ribs, will keep to ay or later. Not handsome in either color or shape, but many who have tested it think it *the finest flavored* they have ever tasted. Tree a good worwer, healthy—spreading. None have been propagated—no name.

The apricot is a seedling of Blenheim, and *blooms several days later*—fruit large, flatish oval, color not quite so dark as the parent; flavor about the same, ripens evenly. Tree a very healthy, rapid grower and free from gum, very prolific—not propagated—no name—originated here.

A lemon colored cling peach is highly thought of in many orchards of this vicinity—large when properly thined, sometimes a little blush in the sun—no red at the seed, which is small—considered our best canning cling. The tree is upright and the most rapid and healthy grower I know, and comes *true from the seed.* The original tree was among a few others planted in "early days" at a mining camp near Mariposa, and lived and bore fruit without fence or care for many years—it was a noted tree—a grand nursery stock.

The other peach is a lemon yellow freestone, medium to large in size, nearly round with a little bright red in the sun—flesh a bright yellow to the seed—very similar to Muir, but more juicy and not quite so sweet—firm, and we like it for canning—carries well—ripens about the first of September—holds well on the tree, which is a tsrong, upright grower. I think it is a cross of the above cling with some freestone. It is known at the hotels along the stage line to "the valley" as "Yosemite," and is the best peach they get. No trees outside of Home Orchard—originated here.

FRANK FEMMONS, Ahwahnee, Madera Co., Cal.

We have not done much cross-fertilizing of plants excepting in Sweet Peas, and on these lines have done a great deal of work since about 1890. The first Cupid discovered by us was in 1894—a pure white. In 1895 we

discovered another Cupid—Pink, or Blanche Ferry. We crossed both, as well as crossing numerous tall varieties, which seemed to break up the strains, and while frequently we would not find any Cupids from such crosses for some years, they were all apt to throw Cupids even though we planted the seed of tall ones. We now have Cupids of every variety that exists in the tall sorts, or more than one hundred.

We saved the first Bush in 1895, although we had seen them before and had not tried to do anything with them. We began crossing, and now have no less than fifty different colors in the Bush. We have ourselves sent out twenty-five tall Sweet Pea novelties, introducing most of them through W. Atlee Burpee & Co., of Philadelphia.

Sweet Peas must be crossed when the bud is quite young before any pollen has caught on the pistil. We have done most of our work by simply taking all the stamens off the plant to be crossed and bringing new pollen to it, although some claim that we should mix the pollen of the plant itself with the new one.

The first year after the seed of the cross is planted it is very apt to show a very inferior reversion to old pink or white, or an inferior red, white or purple. The plants, however, planted again the second year begin to show new strains. This is not always so, since sometimes we get a handsome flower the first year, but it is never a fixed type, and the year following is sure to break.

The majority of novelties, however, are not developed by cross-fertilization, but by selection. There is always something a little different from its fellows in any field of plants, and a selection can be developed to create a considerable change.

The Morse Lettuce is a selection of white seeding plants out of Black Seeded Simpson and then developed for a hardier variety. Pink Prizetaker Onion is a selection out of the common Yellow Prizetaker, and so on. In developing stocks this way we always keep each plant separate, as some plants will produce their kind and others will not do so, and if they are mixed we might never be able to get a fixed stock. In one strain of Sweet Pea, for instance, this year we have 270 individual selections, and we have had as high as 305.

On all of the standard Onions, Lettuces and Carrots we make an extra selection of one hundred plants, roots or bulbs, and from this hundred we save ten individuals and mix the other ninety. Out of the ten individuals we choose one for a breeder, and then carry the work on again from that with our hundreds and tens. This is to breed up a pure stock.

In breeding novelties it is very important that one has a large trial ground so as to try stocks from other sources. We have seen a good many people enthusiastic over something that was new to them, though it was not a novelty.

We believe that nearly all vegetables or flowers can be developed along certain natural lines purely by process of selection—we mean that they can be made early or late, large or small by this method.

Crossing and hybridizing is very slow and discouraging work. We have never done very much of it in vegetables except to try it in Salsify. We have tried it somewhat in Sunflowers, Hollyhocks and Centaureas, but without success so far.

C. C. Morse & Co.,
Per Lester L. Morse, Pres. and Mgr.

The most noteworthy hybrid I have raised is the Canna Mrs. Kate Gray. This canna was raised at Alhambra in the summer of 1896. Italia was the seed-bearing parent crossed with the pollen from Madam Crozy, the pollen from the leaf stamen being used. One seed was obtained, and since that time neither Italia nor Mrs. Kate Gray can be induced with me to perfect seed.

I have a very good hybrid Asparagus which is the result of a cross between Asparagus decumbens, seed-bearing, and pollen from Asparagus tenuissimus.

Some seedling Roses which have flowered giving great promise of good varieties are American Beauty crossed with Kaiserin Augusta Victoria and Papa Gontier crossed with American Beauty.

Pollen grains vary in size and vitality, though they may have been grown in the same stamen. In fact, I am selecting my pollen grains. Method used: A piece of unglazed paper is used, shaking the ripe pollen onto it and curving the paper, and at the same time elevating one end so that the pollen runs down onto a plate. On looking at the pollen with a lens we find that a certain amount of inferior grains are left on the paper, and by repeating the operation only the heaviest grains reach the plate. From experience I have found that these selected grains carry with them the general make-up of the plant bearing them, unless the energy of the stigma overpowers the pollen life. If such is the case the progeny is intermediate between the two in all points. But if the vigor of the pollen predominates it carries with it all the characteristic traits of its parent except color, which invariably leans to the seed-bearing parent. Mixed pollens from differently colored flowers produced all flaked flowers in Gladiolus. But one grain of pollen from a red-flowered Gladiolus on a light salmon-colored one produced a most beautiful pink. The few remaining seed gave results inferior to both parents.

Moisture is fatal to pollen and weakens the vigor of the stigma. It is owing to the dry air of California that California seeds have such sound germinating powers.

All plant life deteriorates, and it is only by crossing variety on variety that we stay this law of nature. In fact, such is the tendency of some varieties to hold to their original that when crossed with pollen from another variety it only adds vigor to the tree or plant.

I have a noticeable instance of this in a hybrid Orange tree. The tree is the result of a cross between the Navel Orange and the Mediterranean Sweet, the former being the seed-bearer. As far as I can see the fruits are perfect Navels in every way. W. H. MORSE.

I have originated five varieties of loquat, which I consider of sufficient value to offer to the public. They are:

Advance, which is very large, sometimes three inches in length; pear shaped; yellow skin; white flesh, and grows often in immense clusters. Ripe rather late in April and May.

Blush, much like the Advance, but somewhat earlier; does not grow in such large clusters; is the least affected of any by the blight, being virtually immune.

Premier; very large; oval; salmon skin and flesh; very early, ripening in March and April, or even before.

Pineapple; a quiet, large, round, white-skinned, white-fleshed loquat of wonderfully rich flavor; grows in very large clusters; ripens with the Advance.

Commercial; exceedingly large; pear shaped; skin, yellow; flesh, white; fruited for the first time in 1900. A very fine loquat, indeed.

The Advance has been recognized as of especial value for about fourteen years. Most of the others are seedlings from it and are of comparatively recent introduction. Up to a quite recent date, my method has been simply to fruit selected seedlings, but now I have some young trees one and two years old, from pits which were hand fertilized, and whose origin is certain. I have fruited over twelve hundred seedlings and have about as many more which will bear in the next two years.

Owing mainly to the introduction of the Advance, the loquat has become very popular in Los Angeles. One man, the past season, sold in that market

one thousand dollars worth of fruit from less than one hundred and fifty trees. There is no reason why the loquat should not become equally popular in the other large cities.

C. P. Taft, Orange, Cal.

I have kept no memoranda, and am unable, in a number of instances, to give the parentage or pedigree of novelties which I have introduced. I have done a great deal of work with Cosmos, but only through selection. Nasturtiums have taken much of my time the last few years. One not yet given to the public, a cross of Phoebe by Sunlight, is very beautiful and most valuable for hybridizing; through it, I expect to develop a tall fringed Nasturtium. The work to which I am most devoted and where I hope to do my best is with Begonias. Considerable time has also been given to Geraniums and Chrysanthemums.

Theodosia B. Shepard, Ventura, Cal.

A STUDY OF GRAPE POLLEN AND WHAT THE RESULTS INDICATE

By N. O. Booth, State Experiment Station, Geneva, N. Y.

The following investigations were carried on in the summer of 1902. Their object was to determine, if possible, the reasons why certain varieties of grapes are self-sterile. It was long supposed that self-fertility in a grape merely meant that the pollen of that variety could not fertilize its own pistil and the earlier students of the botany of the grape taught that such varieties would be told by the recurved stamens which were always supposed to go with self-sterility.[1] Later investigations, which showed that certain of the self-sterile varieties have long stamens,[2] indicated that the recurving of the stamens was not the cause nor even a character which always accompanied self-sterility. Leaving out of consideration the recurving of the stamens as a mechanical cause preventing pollen of a grape blossom from falling on its own pistil and we have four other possible causes remaining: (1) What is known to botanists as dichogamy or the pistil and pollen from the same blossoms, and usually same plant, maturing at different periods; (2) lack of affinity between pollen and pistil of the same plant; (3) the pollen on the self-sterile sorts might be so scanty as to render fertilization improbable; (4) lack of viability in the pollen itself, making it impotent not only on its own pistil, but also on all others. For reasons which will be discussed in the latter part of this paper it was considered that the first and second of these possible causes were not probable ones and our efforts were confined wholly to investigating the third and fourth causes. This year observations were made on a great many different varieties as to the amount of pollen present. All of these estimates were, of course, approximate, since pollen is a material which it is impossible to measure exactly. These observations were made both with naked eye and simple lens. There were a great many variations, but the variations did not appear to be particularly significant. There were greater variations on different clusters of the same vine than normally appeared on different vines of different varieties. The last clusters of flowers to bloom, and sometimes the first, are usually not so well supplied with pollen as those which appear at the height of the blossoming season. Vines just coming into bearing and having only one or two clusters on the vine were usually scantily supplied with pollen. With some of the varieties even where there was no apparent cause in the condition of the vine, the amount of pollen present was apparently insufficient to make pollination at all certain. However, with most of the self-sterile varieties the pollen was quite plentiful and apparently quite sufficient for pollinating pur-

[1]Englemann, Bushberg Catalogue, page 7, ed. 1895.
[2]Beach. Bul. 157, N. Y. E. S.

poses. At the same time that these observations were being made the pollen itself was being studied to determine, if possible, the exact status of the pollen itself as a factor bearing on fertilization. This part of the work was wholly of a laboratory and microscopic nature, the only portion which took place in the field being the gathering of the blossoms. For this purpose the following list of varieties was selected for examination and comparison:

PERCENTAGE SELF-FERTILE VARIETIES.				PERCENTAGE SELF-STERILE VARIETIES.			
	No.				No.		
Riparia...×	1	Clevener	*4	Rip........	1	Clinton	2
Rip......×	3	Marion	3	Rip......×	2	Janesville	1
Rip......×	3	Elvibach	4	Rip......×	3	Berckmans	1
Labrusca.×	4	Aminia	4	Lab......×	4	Agawam	2
Rip......×	5	Grein Golden	4	Rip......×	5	Mo. Reisling	2
Lab......×	6	Barry	4	Lab......×	6	Rogers' No. 32	2
Lab........	7	Wyoming	4	Lab......×	7	Lucille	1
Lab......×	8	Black Eagle	4	Lab......×	8	Triumph	2
Lab......×	9	Massasoit	4	Lab......×	9	Brilliant	2
Lab......×	10	Roscoe	4	Lab......×	10	Lindmar	2
Lin......×	11	Hexamer	4	Lin......×	11	Bailey	2

*For the significance of these numbers following the names see Bul. 157, N. Y. E. S.

Here we have a comparison in each instance of two varieties blooming at the same time or nearly the same time with similar parentage, but one variety being self-sterile and the other self-fertile, the object being to eliminate so far as possible all differences which might be due to species or strain. This investigation naturally divided itself into two lines; first, trial of the pollen in sugar solution to see if it would germinate; second, examination of the pollen under a microscope to see if there were any constant morphological differences between that of the self-sterile and self-fertile varieties. In the beginning of this work we were handicapped by a lack of experience in growing grape pollen and a consequent lack of knowledge as to the best solutions in which to grow it, the literature of this subject being not readily accessible. On this account the results from the first four kinds of pollen tested were uncertain. This was due to using a sugar solution which was too weak for this kind of pollen to make a good growth (1 per cent.). The only differences shown in this pollen was in the budding[1] as none germinated. The Clinton and Janesville both showed buds on from 5 to 10 per cent. of the grains, but the Clevener and Marion showed at the end of four days no change from their condition at time when placed in solution. On June 18, 1902, pollen of Elvibach, Berckmans, Aminia and Agawam were placed in hanging drops of 2½ per cent. sugar solution. They were examined for three successive days thereafter, and the number of germinations noted. The Elvibach and Aminia pollen did not germinate or even bud. About 4 per cent. of the Berckmans germinated and about 10 per cent of the Agawam. On the twenty-first pollen was prepared as before except for using a 5 per cent. solution of dextrose in place of the previous medium. The varieties from which pollen was taken in this instance were Grein Golden, Mo. Reisling, Barry, Rogers' No. 32, Wyoming and Lucile. Notes were taken on these cultures on the 23d, at which time they were in the height of their growth, none germinating after that date. At this time the pollen of the Grein Golden,

[1]The first stage in pollen germination.

Barry and Wyoming had not changed in any way from their condition when they were first placed in the solution. About 12 per cent. of the Mo. Reisling germinated 20 per cent. of Rogers' No. 32 and 10 per cent. of the Lucile. Pollen of these same varieties was placed at this later date in 10 per cent. dextrose solution with the following results: Grein Golden out of an estimated 200 grains one made a very weak growth. Barry and Wyoming not changed in any way. Mo. Reisling, about 15 per cent. germinated. Rogers' No. 32, about 50 per cent. and especially strong. Lucile was accidentally destroyed. The same varieties were tried in 10 per cent. sugar solution with the following details: Grein Golden, out of an estimated 150 grains one germinated, growth short and weak. Mo. Reisling, 20 per cent. grew. Barry, none. Rogers' No. 32, about 75 per cent. good and strong. Lucile is a mass of growth at least 80 per cent. In 20 per cent. sugar, Grein Golden shows 2 in approximately 250. Mo. Reisling, 25 per cent. Barry, none. Rogers' No. 32, 90 per cent. Wyoming, about 5 per cent., growth weak. Lucile, 95 per cent.

On July 2 cultures were made in 20 per cent. sugar solution with pollen of Black Eagle, Triumph, Massasoit, Brilliant, Roscoe, Lindmar, Hexamer and Bailey. Notes taken the following day show: Black Eagle, none budded or grown. Triumph, practically all budded but only about 10 per cent. grown to any length. Massasoit, little budding and no growth. Brilliant, all budded, 60 per cent. grown. Roscoe, none budded and none grown. Lindmar, about 60 per cent. budded and 10 per cent. grown. Hexamer, no buds and no growth. Bailey, about 60 per cent. budded and 5 per cent grown. The foregoing results are tabulated below:

Name of variety.	Class.*	Fertility or sterility as determined by previous field experiments.	Date when placed in solution.	Date when examined.	Solution used for examination.	Per cent. of germinations.
Elvibach	4	Sterile	June 18	June 19	2½% sugar	None
Berckmans	1	Fertile	"	"	" "	4%
Aminia	4	Sterile	"	"	" "	None
Grein Golden	4	Sterile	June 21	June 23	20% sugar	10%
Agawam	2	Fertile	"	"	" "	1% weak
Mo. Reisling	2	Fertile	"	"	" "	25%
Barry	4	Sterile	"	"	" "	None
Rogers, No. 32	2	Fertile	"	"	" "	90%
Wyoming	4	Sterile	"	"	" "	5%
Lucile	2	Fertile	"	"	" "	95%, very strong
Black Eagle	4	Sterile	July 3	July 5	" "	None
Triumph	2	Fertile	"	"	" "	10%
Massasoit	4	Sterile	"	"	" "	None
Brilliant	2	Fertile	"	"	" "	60%
Roscoe	4	Sterile	"	"	" "	None
Lindmar	2	Fertile	"	"	" "	10%
Hexamer	4	Sterile	"	"	" "	None
Bailey	2	Fertile	"	"	" "	5%

*See Bul. 157, N. Y. Ex. Str.

Besides these there were some other differences between the self-strile[1] and the self-fertile pollen which seemed to be constant. The self-fertile grains seem to be surrounded by a mucilaginous substance which makes

[1]I retain the use of these terms self-sterile and self-fertile all through this bulletin, although the results show they do not express the whole truth.

them stick to one another more or less so that the pollen, whether it lies dry on the slide or is placed in liquid media, arranges itself in a succession of clumps. This mucilaginous substance does not appear to be soluble in water as the pollen grains retain their position even after several days in the solutions. The self-sterile pollen, on the other hand, shows no such arrangement, but the grains distribute themselves either on the slide or in the liquid like so much dry powder, quite by chance.

The next phase of the work was the microscopical examination of the pollen grains to see if there were any characteristic differences in the size or shape of the different classes of pollen. All pollen, whatever its shape may be when it comes from the anther, swells on contact with water and most other liquids, assuming a spherical shape. The results of this part of the work can be better illustrated than told. On the following pages are cuts which are reproduced from photo-micrographs of the pollen mounted in balsam. The characteristic differences are very apparent. The self-fertile forms are oblong, blunt at the ends and quite symmetrical. The self-sterile sorts as may be seen are quite different in shape, being more irregular and showing little of the symmetry of the other class. Pollen from all other varieties in the list previously given showed these same shapes according to the class to which the variety in question belonged, but the blooming season of the first eight varieties was past before I thought of illustrating this phase of the work, and later the balsam mounts of Roscoe and Lindmar were accidentally destroyed.

Examination of pollen, from varieties of grapes which had given conflicting results in Prof. Beach's work to determine if they were self-sterile, showed that these forms may be mixed. Eaton was the first one of these which was examined on June 26. The pollen of this variety is quite irregular in shape and size, and only about 10 per cent. show the regular self-fertile shape, although there are numerous others which approach it very closely. They are considerably larger than average pollen. In 20 per cent. sugar solution about 15 per cent. of this pollen germinated. None of the growths appeared healthy, however, or at least were not like those of completely self-fertile pollen. In normal self-fertile pollen the tubes formed on germination are approximately the same size throughout, but the tubes of Eaton varied in size at different points of their course, being restricted at one place and swollen and distorted at another. The tubes were fully as long as those of normal self-fertile pollen. Other pollen of the same class which Prof. Beach brought for testing were Dracut, Amber, Maxatawney, Faith, Geneva, Montefiore, Caronicus, Oneida, Gold Dust and White Jewel. These were tested, as were also pollen of Red Traumener, Red Veltliner and Chables sent by Dr. Tinker, of New Philadelphia, Ohio. In each instance the percentage of pollen which germinated did not vary widely from the percentage of self-fertile forms which the microscope showed that particular variety to contain. And it appears very certain that the capacity of the pollen for growth is in direct proportion to the percentage of self-fertile forms present and their conformity to the self-fertile type. Pollen of various varieties of

grapes varies considerably in size, but there is no apparent connection between the size and germinating capacity.

From these results it appears that one of the reasons why certain varieties of grapes are self-sterile is a lack of viability or potency in the pollen itself.[1] There may be other minor factors (quantity of pollen produced, etc.), but this is sufficient to account for all of the phenomena observed in the field. There are no distinct classes of self-fertile and self-sterile forms, but all gradations exist from one extreme (pseudostaminate) to the other (pseudo-pistillate). It further appears that pollen from the same variety may vary in different years and in the same year in different localities.[2] This gradation of pollen is quite readily noticed on examining pollen from a dozen or two of varieties selected by chance. In selecting the varieties which are given in the list in this bulletin extreme types were purposely sought so that any differences which might exist would be most apparent.

The grape is evidently, so far as its phænological characters are concerned, now in a state of evolution from an assumed older hermaphrodite[3] form to forms that are essentially staminate and pistillate. All of the staminate forms which I have observed have small abortive pistils which conform with the observations of Engelmann. Others report staminate flowers with no trace of pistil remaining. On the other hand, the most advanced pistillate forms still retain their stamens and pollen although so far as their true function is concerned they are abortive. There is considerable corroborative evidence as to this being an incomplete evolution in the fact that the forms are not fixed and separate, but blend together and are quite unstable even on the same plant.[4] These facts seem to show that our grape is in a state of very unstable equilibrium coming from an ancestry of diverse sexual types.[5]

It might be interesting to consider the probable cause of this evolution. It seems reasonable to suppose that there must be some advantage which the staminate and pistillate vines have over the older hermaphrodite forms or they would not have developed and persisted. This advantage probably lies in the fact that cross fertilization is thus assured.[6] The seedlings resulting from cross fertilization being usually the stronger[7] would have the better chance in the struggle for existence with those from vines which were self-fertilized. However, we should not lose sight of the fact that there are also some advantages to the hermaphrodite forms, and the chief one of these lies in the greater certainty of fertilization and consequent seed production. Where vines are widely scattered, the hermaphrodites would have the ad-

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[1]Such plants are well known to botanists, and are called pseudo-hermaphrodite. Kerner & Oliver: National History of Plants, 291.

[2]Beach. Bul. 157.

[3]Bailey. Survival of Unlike, 347, and Geddes & Thompson Evolution of Sex.

[4]There is a vine at this Station which bears both staminate and hermaphrodite flowers. Mr. N. B. White, Norwood, Mass., reports a male rip. × lab. vine on his ground which has fruited but twice in the last 30 years. See also Bushberg Catalogue, 8.

[5]Kerner & Oliver, Natural History of Plants, 300.

[6]Kerner & Oliver; *l. c.*

[7]Darwin. Cross Fertilization of Plants.

vantage since the chances of cross-fertilization of staminate and pistillate forms under such circumstances would be remote. Where the conditions are such that vines are numerous and closely adjacent, the opposite would be the case, as fertilization of the pistillate flowers would be comparatively certain and the seedlings resulting would have the advantage over those resulting from self-fertilized hermaphrodites. It must be remembered that the adjacency referred to is not merely a matter of distance but would be modified more or less by other factors, such as number and kind of insects normally present, direction of wind, surrounding vegetation, etc. It must also be remembered that although pistillate flowers are necessarily cross-fertilized it does not follow that hermaphrodite flowers are self-fertilized. These may be cross-fertilized also either by other hermaphrodites or by staminates, and the pistallate forms may be pollinated by either the staminates or hermaphrodites. In any of these cases the resulting seedling, while it would possess the individual vigor due to crossing might be itself in any class so far as its phænological characters are concerned. This mixing and the fact that the advantages of each class tend to a certain extent to balance each other probably account for it that neither form has supplanted the other but both are still present. In reference to the question referred to in the first part of this note as to whether dichogomy or the maturing of stamens and pistils on the same plant at different periods might exist in the grape, observations seem to show that this is not the case. The anthers usually burst and the pollen is liberated before the pistils become receptive, but a good portion of the pollen remains on the anther and is released gradually even some time after the pistils are in a condition to be fertilized. Grape pollen is notably resistant to the ordinary influences of decay[1] and it can be readily seen how in an inconspicuously flowered plant like the grape, where insect visits might not be so numerous as would be desired for pollinating purposes, keeping qualities on the part of the pollen grains would be so valuable that they could not be sacrificed even for so important a consideration as cross-fertilization. The question as to whether there might not be a lack of affinity between the pollen of a self-fertile grape and its own pistil will be difficult to settle conclusively. What evidence we have seems to show that this does not exist.[2] Trial of the pollen of self-sterile varieties on their own pistils and on the pistils of self-fertile varieties while subject to such accidents as are liable to occur in such delicate work as this[3] seem to show that the fault lies in the condition of the pollen and not in any relation which exists between the pollen and pistil. Furthermore, the fact that the phænological evolution referred to ever took place is strong negative proof that lack of affinity does not exist. If any of the original hermaphrodite forms has possessed that quality by which pollen of a certain plant was impotent on the pistils of the same plant then there would have been no cause to produce the

[1]Bul. 157, N. Y. Ex. Sta., 438. Pollen was germinated by the writer three weeks after it had been gathered.

[2]Beach, S. A. Self-Fertility of the Grape. Investigations of 1900 and 1902. Bul. N. Y. Exp. Sta. (in preparation).

[3]Bul. 157, N. Y. Exp. Sta.

staminate and pistillate forms of to-day, since cross-fertilization would have already been assured.

The economic bearing of these results is quite apparent. In choosing fertilizers for self-sterile grapes it will give us a means of selecting the best varieties for this purpose without the long vinyard trials by sacking, etc., which have heretofore been necessary. To the hybridist it may be of service and it provides a means of determining the sexual status of varieties within a single blossoming season.

SUMMARY.

I.—The self-sterility which is known to exist among many varieties of cultivated grapes is frequently, if not always, due to a lack of potency in the pollen.

II.—This lack of potency is indicated in the pollen grains by a shape which is quite different from that of potent pollen.

III.—It is also shown in the arrangement of the pollen either dry or in liquid media.

IV.—Certain varieties of grapes bear pollen in which both the potent and impotent forms are mixed. Trial of this mixed pollen shows that the amount which germinates is approximately in proportion to the potent forms present.

SOME HYBRID NICOTIANAS

By Ph. de Vilmorin, Paris, France

Although the culture of tobacco is strictly limited in France on account of the State monopoly, some species of Nicotiana and even the N. tabacum are grown for ornamental purposes.

Amongst the more popular are Nicotiana sylvestris, N. affinis, and a deep red variety of N. tabacum, of the Maryland type, known under different names, none of which is good. We tried at Verrieres some crosses between these kinds with the object of improving their value as garden flowers, and the following is a brief account of the results:

First Case: *Nicotiana sylvestris and red N. tabacum.* The cross was made in 1899, and gave two types in 1900:

A. Apparently pure sylvestris (dropped).

B. Very distinct hybrid, having the general appearance and the foliage of Nicotiana tabacum, but branched stems, flowers pale red, distinctly longer than those of N. tabacum, very handsome plant giving flowers in abundance during the whole summer. No seeds, except on a few late flowers. These seeds produced in 1901:

(*a*) 19 out of 20 plants of pure sylvestris (although from seeds of a red flowered hybrid), the seeds of which gave again pure sylvestris in 1902.

(*b*) Only one plant different from the type by its foliage smaller and longer, dull green, its flowers 6 centimeters long (10 cm. in N. sylvestris) and horizontal (drooping in N. sylvestris) That plant gave seeds and

(*bb*) in 1901 was the origin of a very striking variation, namely: All the plants differ more or less from one another although they are all closely related to N. sylvestris. Some are taller than N. sylvestris; 4 of them are dwarfer (60 centimeters to 1 m.); some having a bright green foliage, some a dull green; and great differences in the shape and size of the foliage.

These 3d generation hybrids have nothing left of the N. tabacum as far as regards the color of flowers. The only proof of their hybrid origin is in the variation in the size of the plants, size, form and color of leaves, and more or less drooping disposition of the flowers.

Second Case: *The same cross* (*N. sylvestris and N. tabacum*) was made again in 1901 with the same result in the first generation, but there were no pure sylvestris amongst the seedlings, and the flowers were somewhat paler,

the N. tabacum (taller) being paler than that used in 1899. Will probably give seeds.

In both cases (1 and 2) the first generation hybrid of N. sylvestris and N. tabacum was a very beautiful and decorative plant. The length of flower stalk and abundance of flowers was increased by the fact that the flowers were sterile or nearly so.

THIRD CASE: I wish to report a third case of the same hybridization, made in 1900 by a very careful observer, Mr. Bellair, head gardener of the public gardens of Versailles, which is exceedingly interesting, not only in itself, but also because it differs from what happened at Verrieres in case 1, and is a proof of the extraordinary diversity in the results of hybridization.

The N. sylvestris and N. tabacum gave in 1901 a number of seedlings, much like one another and also very like one we have at Verrieres. Mr. Bellair used them for decoration in the Parc de Versailles, and wrote an article about them in the Revue Horticole, of December 1st, 1901. At that time he stated that the plants were sterile except a few of them which were very like N. sylvestris (same thing occurred at Verrieres). Nevertheless he could find a few good seeds on the distinct hybrids and got a number of new forms, none of them being like either parent, but all intermediate and generally smaller than either. Mr. Bellair sends me the descriptions of four forms.

A. Dwarf plant, 90 cm. to 1 m. high, flowers horizontal, 10 cm. long, 42-45 mm. broad, foliage dull green, somewhat like that of Nicotiana virginica.

B. Plant 1 m. 25 high, very stiff, leaves small, flowers looking upwards, about 6 m. long and 3 m. broad.

C. Plant interesting from the lengths of the flowers (14 cm. instead of 10 cm. in N. sylvestris) which are, however, not very broad. Some of the leaves are long, narrow and drooping, whilst the others have normal proportions.

D. Plant 1 m. 40 high, very like N. tabacum, but flowers broader, and of pale rose that by and by becomes almost white.

These four types, says Mr. Bellair, were selected from the bed when still mere seedlings, because they already showed characters distinct from the lot, and they were planted in the garden.

It was, however, among the plants left in pots that Mr. Bellair was fortunate enough to find a few rose N. sylvestris, that is plants almost like N. sylvestris, with flowers only a little shorter and of rose color.

It is interesting to notice that the plants here described are second generation hybrids, when it is quite expected to find the phenomenon of "variation desordonné," as it was called by Naudin. The forms A, B, and C are exceedingly like the ones we got at Verrieres from the same cross but only in the third generation. Besides, Mr. Bellair was fortunate enough to get plants with colored flowers, which was not our luck. (See case 1.)

FOURTH CASE: *N. tabacum and N. sylvestris.* This cross had been tried in 1900 by Mr. Daveau, head gardener in the botanical garden at Montpelier. The hybrid is described in the Revue Horticole, December 1st, 1901, it is a beautiful plant forming thick bouquets of flowers and absolutely sterile. The

same cross was made at Verrieres, in 1901, and I can report only the results of the first generation, which are very different from Mr. Daveau's hybrid: Plants very strong and high (2 m. 50), general appearance of some of the varieties of N. tabacum used for tobacco making, flowers with a long tube (6 cm.) small and pointed petals, pale rose, forming a loose panicle; the flowers and are thus intermediate between those of the N. sylvestris and N. tabacum. Seems to give seeds.

FIFTH CASE: *N. tabacum (very deep red variety)* and *N. affinis.* Cross made in 1899, gave a plant very like N. affinis in general bearing and shape of flowers, but dwarfer and not so strong. The flowers are smaller and a little shorter, sometimes (on the same plant) pure white, sometimes rose on the outside and white inside, sometimes rose or varigated in the inside. This very interesting hybrid is unfortunately absolutely sterile; all attempts at self-fertilizing or crossing with other varieties or species were complete failures. Since 1900 it has been propagated by root cuttings.

SIXTH CASE: *Nicotiana glauca and N. tabacum.* Cross made in 1901. In 1902 gave two forms:

> A. 11 plants (out of 12), quite like one another, tall (2 m.), branched, leaves oval; flowers small, greenish yellow when young and afterwards rose; these plants are evident hybrids. More closely related to N. tabacum than to N. glauca.
>
> B. The twelfth plant quite distinct, 80 cm. high, flowers very short, petals rounded, yellowish green, seeds abundant. That plant is different from the eleven others resulting from the same cross and also from both parents, which fact it is very difficult to account for, especially in the first generation.

This last hybridization was made solely as an experiment, there being no chance of improving N. tabacum by a cross with N. glauca.

CONCLUSIONS.

(1) In all cases first generations have been very infertile, giving only a few seeds from late flowers, except in case 5 where the hybrid is completely sterile and in the form B of case 6 where the hybrid is very fertile.

(2) In some cases the plants of the first generation are intermediate between the parents and entirely like one another except in case 1 wherein was noticed a slight retrogression towards the mother and again in case 6 wherein the form B is very distinct from both parents.

(3) The first generation hybrids were more closely related to the mother than to the father in case 4 and the reverse in cases 1, 2, 3, 5 and 6, but in cases 1 and 2, where the second and third generations were observed there is a distinct retrogression towards the mother.

(4) Generally the hybrids are neither so tall nor so strong as their parents, but, because of their sterility, have a splendid and long enduring inflorescence.

I acknowledge that these conclusions are vague and unsatisfactory, but I must be content with them for the present. I hope that further experiments may help to a true knowledge of the transmission of characters in hybrids.

EVER-BEARING STRAWBERRIES

By P. de Vilmorin, Paris, France

I have very little to add to the history of everbearing strawberries as it was published by my father in 1898 (Journal of Royal Horticultural Society) beyond what has since been done at Verrieres in the way of improving the varieties described.

Within the last few years the St. Joseph and especially the St. Antoine de Padoue strawberries have proved to be very good and prolific plants. Wherever the water supply is sufficient to keep them growing they gave all through the summer and as late as October a great crop of large, well-flavored fruits, fetching high prices when sent to the market. Nevertheless the size of the late fruits cannot be compared with that of some of the big June strawberries, and our object has been to increase that size.

Crosses were made in 1897 by my father between St. Joseph and different varieties of the large-fruited strawberry. Almost all of them resulted in the production of more or less everbearing plants. One with Edouard Leport proved to be specially good; it had large fruits and was propagated for distribution.

In 1898 more crosses were tried with St. Joseph and the result was the same. One seedling (St. Joseph × Noble) much better than the others, with very large and well-shaped fruits, will very likely become a good garden variety.

Among the curious remarks afforded by the study of the hybrid strawberries one is especially striking and shows well how difficult it is to foresee the result of a hybridization. A cross made in 1898 between St. Joseph and Louis Gauthier—two everbearing varieties—gave birth to a plant that was *not* everbearing.

In 1899, 1900 and 1901 we worked on the same lines as before, only using St. Antoine de Padoue instead of St. Joseph. Many seedlings are under observation now and give good hopes for the future.

It is perhaps not useless to remark that all these large-fruited everbearing strawberries give bigger and nicer fruits on the preceding year's runner than on the older plants, the latter being generally exhausted by their abnormal production.

In order to increase the strength of the large-fruited varieties and their resistance to fungi my father had the happy thought of crossing them with the Fragaria sandwicensis, a very healthy and vigorous species. A cross made two years ago between St. Joseph × F. sandwicensis gave a plant as strong as its father, with dark green leaves; the mother contributed the everbearing character and the flavor to the plant.

I am in hopes that this may mark the beginning of a new era in strawberry breeding.

SOME POSSIBILITIES

By C. L. Allen, Floral Park, Long Island, N. Y.

Plant breeding is usually regarded as the act of reproduction, the perpetuation of a given species, or type, that each plant acts as a machine acts without volition, producing, because it was made to produce, wherever placed in the order of creation, and that variation, in form or character, was the result of external circumstances being more or less favorable to the development of the machine as regards strength and recuperative energy, which, is in a great measure, true, but not the whole truth, as we shall endeavor to show.

To us, plant breeding has a much broader significance; it is education—the giving of character to—the bringing of the plant up to the highest possibilities of its creation, which are, to a great extent, bounded by the opportunities afforded it.

To fully understand the plant's capabilities for development it will be necessary to first study them in their native habitat; to do that it will be necessary to first take a glance at their geographical distribution, then note the variation in form, substance and habit incident upon a changed condition of climate, as well as in the character of soil when removed to distant localities.

Buffon, in speaking of the geographical distribution of plants, says: "The vegetation which covers the earth, and which is still more attached to it than the animals which browse it, are even more interested than they in the nature of climate." Each country, each changing degree of temperature, has its particular plants. We find at the foot of the Alps the plants of France and Italy; at their summit we find the plants of the frozen North, and the same Northern plants we find again at the summit of the mountains of Africa. Upon the range of the hills which separates the Mogul Empire from the Kingdom of Cashmere, we find on the southern slope many of the plants of the Indies, and it is not without surprise that we find on the north flanks many of those of Europe. It is also from the extremes of climate that we draw our drugs, perfumes and poisons, and all the plants whose properties are in excess. Temperate climates, on the contrary, only produce temperate things; the mildest of herbs, the most wholesome of legumes, the most refreshing of fruits, the quietest of animals, the most polished of men, are the heritage of the mildest climates.

In the natural distribution of plants, temperature was the principle that governed selection; it might be more proper to say that each was created for the place it was to occupy, but with the power of adaptation to changed conditions of soil and climate, to a considerable extent.

As a principle, plant breeding is plant education, or plant development, in the order of evolution.

While we can readily understand the plant's changes in form or substance, through climatic influences, they are frequently so marked in their essential character, that no one can understand these variations unless they are the results of the plant's own volition.

Upon general principles, it is safe to assert that plant breeding is simply giving a plant an opportunity to develop its latent forces, in obedience to the command to grow. A plant in its native habitat, is an undeveloped, uneducated object in the realm of nature, and has its analogy in the wild man of the wood, from which it differs only in degree. In its native state, the plant's only mission is reproduction, its whole energy is along those lines. The development of its functions, or active principle that gives it a place in the economy of nature, whether it be for food, raiment, or medicine, or for its uses in the mechanic arts, is left until such times as these productions become indispensable to other creations.

As a fitting illustration of this principle, we will take the banana, Musa paradisica, the fruit of which is highly nutritious and is credited with sustaining a larger number of the human race than any one of the cereals. Though less nutritious than wheat or potatoes, yet the space occupied by their culture, and the care required are so very much less that Humboldt has calculated the produce of bananas compared with that of wheat as 133 to 1, and to that of potatoes as 44 to 1.

In its native habitat, the fruit is filled with small, black, shining seeds, which, like all other seeds, exhaust the plant's vital energies, far more than the fruit we eat.

Under cultivation, the banana rarely ever produces seeds, and reproduction is effected by suckers, or more properly, off-sets, and the energy required to produce seed is applied to the production of fruit, in remuneration for services rendered in the reproduction of the plant. An individual plant of the banana produces but one crop of fruit. As soon as this is gathered, the stem immediately begins to decay, and is removed, and the numerous off-sets from the base of the plant are separated and planted out in new fields, and, in a few months will produce a crop of fruit, which keeps up a continuous harvest.

The orange is another striking illustration of the plant's adaptation to man's use. In the direct line of evolution, it ceased, but a few years ago, in one of the districts of Brazil, where this fruit is found growing to the greatest perfection, to produce seed, instead of which, the whole energy of the tree was employed in producing fruit, nature, seemingly, confident that the perpetuation of the species was safe in the hands of those who were to profit by the fruit produced. The result was that a much larger crop was obtained from each tree, and of superior quality, as well as increased size.

The seedless orange, as now grown in California, is not only the largest, but the most delicious fruit of its kind that comes to our market. No horticulturist can claim the honor of originating the seedless orange, no one had the least idea of such a possibility.

The potato, where grown to the greatest perfection, rarely produces seed,

other than through the agency of artificial fertilization. Nature, working along the lines of that rigid economy that marks her every action, has thrown the energy required for the production of seed, into the development of the tubers, which are furnished with buds for the perpetuation of a given type which the seed could not do.

So far as we know, nature never did produce a vegetable fit for developed man to eat, but in every class there were the possibilities of the greatest usefulness, when, or where, their uses were required. To reproduce is nature's work; to develop or educate was a duty imposed on man.

In the climate of Sweden, where there are but nine weeks of spring, summer and autumn, the active principle of growth is so intense during that short period that their meadows yield two crops of the most nutritious grass, and their gardens two crops of the most delicate and delicious vegetables. Do not understand me to say that they can take seeds grown in a more southerly clime and get such results, at first; they cannot, but by slow stages the plants have become adapted to localities where rapid growth is required, until the results are as stated.

On the contrary, seeds grown in Denmark will, if planted in a more southern locality, make for a season a much more rapid growth, but a second or third generation will take the full time allowance for reproduction.

Many plants which are annual and herbaceous in temperate climates, become perennial and ligneous in the tropics, and the reverse, a fact that is the cause of some strange freaks in plant variation. Take as an example, the common castor oil plant, which is here grown as an annual, and now produces its seed freely; when first introduced, it was a tall growing plant, and rarely ripened but the fruit of its first flowers. By saving the seed for a few generations, it adapted itself to the climate, became more dwarf in habit, its seeds grew smaller, and the plant is now extensively grown for commercial purposes. The same species in Africa grows to an immense size, and is perennial in habit, while its trunk is as woody as most of the forest trees.

A better illustration, from the fact of its being one in which we are more interested, may be found in the lima bean (Phaseolus lunatus). In South America, where it is indigenous, it is a tender herbaceous perennial, a most rampant grower, and, having fully nine months to perfect its fruit, it can ramble at leisure without fear of frost. When spring time comes, as it does everywhere, from the crown of its immense fleshy tubers, not unlike the Chinese yam, som eof which weigh fully fifty pounds each, there shoot forth numerous tender stems, not unlike the perennial Ipomoeas, which make a rapid growth, and, twining over other vegetable forms, becomes an impenetrable mass.

Like most other twining plants, it follows the sun in its course, which is there from right to left, directly opposite from the natural direction of twining plants north of the equator, which accounts for the difficulty experienced here in getting them to climb the poles. Habit says, go to the left, the sun says, follow me and go to the right, the result is it will not willingly do either.

The farther removed from its southern home, the more rapid its growth and the more dwarf its habit, until it reaches the limit of growth or time it has

for growth sufficient for reproduction. By slow stages, in its adaptation to changed conditions, it has assumed a dwarf, bushy habit, instead of a climbing plant, as we find it where indigenous.

The question—What is the cause of this change in habit, where and how did the bush limas originate, is readily understood to be in the direct line of evolution.

There is not, neither has there been, a greater change in the habits of growth of the lima bean than in the quality of the seed produced as an article of food. The large, flat lima grown at the South is far more delicious and tender than that grown at the farthest point North where the same can be produuced. The beans brought from Lima would not reproduce themselves if planted in the Northern and Eastern States, and our valued productions are the results of gradual removals from South to North by slow stages.

THE SCARLET RUNNER.

What is true in regard to the lima bean is equally true with the scarlet runner, and allied species, also a native of South America and perennial in habit. Here it is grown as an annual, but few, relatively, know it to be otherwise. We are well acquainted with a plant which has been growing in a border from a seed planted ten years ago. This plant is close to a wall on the south side of the house, which is kept so warm by the furnace in the cellar that no frost ever enters the border. It throws up a large number of shoots annually and produces its flowers and seeds in great profusion. We examined its underground stem, or tuber, last spring and found it as large as a mangel wurzel. It is but correct to say that each one of the stems is not as prolific as a single stem when grown as an annual.

CORN.

There are but few of our useful plants so greatly changed by a change of locality as the different varieties of field corn. Not only is this the case as to the period of time the crop takes to mature, but to the essential character of the grain when grown under changed conditions of soil and climate. Corn has been, and is being, grown to a profit, where there is rarely a month in the year without a frost. I have seen it growing in the province of Quebec, where such climatic conditions existed. The stalks did not exceed more than four feet in height and of proportionate diameter, yet nearly all of them produced each two small ears of sound yellow corn, of most excellent quality.

Some of this corn was taken to Central New York and given every attention necessary for the production of a crop, and never did plants respond more freely to good treatment. The growth was no larger than the same made in Quebec, and the harvest was made in about the same number of days after planting the crop, being harvested before the middle of August.

The seed product was all used for planting the following season, but, having found that in the climate of its adopted home, it had twice as long a time to mature, it took it all and grew as high and strong as the yellow flint corn there generally grown, and produced as large ears. Its identity as an early type or variety was lost, but the lesson taught was instructive and valuable, showing how readily the plant adapted itself to the conditions as found and how readily all plants accept the situation given them.

SWEET PEAS.

In its native habitat, the sweet pea is the tallest grower of the numerous genus of Lathyrus, and, where growth is slow, and climate moist, its period of flowering is much longer than where the seasons are short and the atmosphere rare with high temperature. But the law of reproduction is always understood and respected, and in obedience to command, it adapts itself to whatever situation in which it may be placed.

In the short seasons of Northern New York, and in the heavy clay soil, with seasons of heat and drought, in order to produce the required number of seeds, growth had to be curtailed, and the season of flowering made shorter; the result was that all its flowers were produced nearly at the same time, which makes the plant far more effective and useful as a florist's flower. This is shown in the Blanche Ferry and the dwarf varieties, now becoming common, to be the result of the plant's adaptation to its environments.

CABBAGE.

The cabbage in cold climates adds an additional number of leaves to its heart, and the colder the climate the greater the number, and the more compact their arrangement. While nature is protecting her own, she at the same time provides for other creations, worthy objects of her care.

Seed grown here, if taken to a warmer climate, in one generation produces soft heads, simply because greater protection is unnecessary for purposes of reproduction.

RUTA BAGA.

The same is true of the ruta baga. In climates where the roots can be left in the field, they invariably produce large, long necks, which are thickly set with buds that will in due time develop the stalks and branches that yield the seed. On Long Island, N. Y., the tendency of the root is to grow globular in shape, and with a little care in selection will become nearly a perfect globe. There is a type that does not show a particle of neck, and when taken up upon the approach of winter, nearly every leaf drops off, so there is no necessity for cutting the tops before trenching. This feature is more noticeable when they are grown in light, sandy soil.

These changes, constantly going on, have made in almost every genus of plants, many types so entirely different in form and habit, from the parent, as to constitute them distinct species. Certainly, the bush lima bean, of the potato type, and the large, flat variety of twining habi, if found indigenous in the same locality, would, by any one with authority to define species, been given distinctive specific names.

There are as many types of vegetables as latitudes in which they are grown, and these will be more or less modified by the character of tho soil in which they are grown. And it is not as arbitrary as we have been taught to suppose.

Having shown that many, if not most, of the changes in vegetable forms have been made necessary for self-preservation, when growing under changed climatic conditions, and also that these changes were along the lines of evolution, to enable them to live in harmony with other creations, we will now briefly consider, what is generally supposed the most important element in plant breeding or plant development, viz.:

HYBRIDIZATION.

Much stress is laid on the importance of hybridization, as an order in the development of species and the parent of new varieties. While we are willing to credit any agency that gives us an improved vegetable form, no matter what it may be, it is possible to give to this more than is justly its due.

When hybridization resulted in a mule, incapable of reproduction, we could only regard it the parent of monstrosities, which would render all such results, from the seedman's standpoint, utterly useless, as any seedless plant would be to those whose business it is to sell seeds, but to-day we accept it as an important agency in the development of vegetable forms.

But how are types developed through this agency? may be asked. We reply by simply uniting the good qualities of two or more vareities into one. As, for instance, a tree, shrub or plant may be vigorous in growth, and strong in reproductive energy, but its fruits may be low in those qualities that give it a commercial value.

An allied species or variety may have but little strength physically, but produce fruits, in limited quantity, that reach the highest degree of perfection. A union of the good qualities of the two is a most valuable acquisition. This is our work—our duty—as agents in plant breeding or plant education.

When these imprisoned energies are set free by cross-fertilization, they are liable to impart to their progeny some very strange combinations, many erratic freaks, present themselves. These strange variations are apt to show themselves for several generations. This makes the work of selection a necessary one, which must be long continued, before a desired type becomes permanent.

That we have secured a variety that will prove constant and reproduce these pronounced characteristics we so highly prize, when first they present themselves, is by no means certain. The most desirable varieties have been the result of a long series of careful selection freom stocks which showed a tendency to improve under favorable circumstances.

Improvement along any line of reproduction is slow, and uncertain. Many of the varieties produced through the agency of cross-fertilization, while of great value in the locality where they originated, are, or may be without value elsewhere, as they do not always reproduce themselves when grown under changed conditions of soil and climate. They may come true to the type the first year, but will not reproduce it, from the fact of its not being congenial to its changed conditions. This is true with all leading varieties, and, while the seed of a given type will come true under nearly all conditions where the plant can be grown at all, yet it may not do to grow it for seed purposes. The seed growers all over the world fully understand these conditions, and get their stocks from localities where development, rather than deterioration, is the natural tendency.

REPRODUCTION.

It matters not when, how or where a given variety was produced; the question for the plant breeder is, where can it be reproduced to the best possible advantage? In other words, where can he procure seeds that will give the greatest satisfaction in the locality he has to supply?

As all plants owe their existence, in the first place, to the seed, reproduction depends wholly upon the seed's power of germination. But germination in its relation to the value of the plant produced, is relative; all depends upon what the plant is grown for. A high power of germination is absolutely necessary in one case, while in another, a low percentage of germination is just as important. Weakness of vital power is highly important in many kinds of plants that are grown for the beauty of their flowers. Take, for instance, the Balsam and Zinnia; the weaker the vitality, the more care nature takes to protect the germ that is to perpetuate the species. The flower is increased in size by an additional number of petals to protect the vital spark that is to perpetuate the species.

It is an open secret that some of the truckers that grow cabbage largely for the New York market never use seed until its germinating power becomes greatly weakened. These men have the reputation of being the most successful growers in the country, and sell seed to their neighbors at an exorbitant price. To their shrewdness must be credited the fact of their giving to their neighbors new seed, which will not always give the desired results. The seed these growers use, would be discarded by any dealer, and by those who seem to have authority to place the value of all seeds upon the test of germination.

The egg plant grown in hot climates produces an enormous quantity of seed of high germinating power, and what would be called a handsome sample. The egg plant grown on Long Island grows to a much larger size, has but few seeds, and these of low germinating power, and have a shrunken, shriveled look, but the growers have no trouble in getting $5 per ounce for the seed, because of the better fruit it produces. An ounce of this seed, that many dealers would reject as worthless, will yield three times the number of barrels of fruit as would the same amount of seed grown at the extreme South.

Seed which has not reached maturity, may, it is true, possess the power of germination, but it will always retain a disposition to disease and weakness. It is true, that disposition may be so far conquered by a coincidence of favorable auspices, and by a soil and temperature peculiarly adapted to the requirements of the plant, that imperfect seeds may produce vigorous and healthy plants; but there is always great danger of the crop failure, and of the progeny's inheriting disease instead of strength.

So far as we have been able to learn, our most practical and intelligent agriculturists who have paid any attention to the subject, are fully convinced that great advantages arise from sowing the largest and most perfect grains of all cereals. That by systematic care in the selection of the largest and most perfect grains, for seed purposes, the development of the types are perfected. The secret of plant breeding, so far as it relates to the cereals, consists in ever breeding from the highest developed and most prolific types.

In choosing the seed, a preference should always be given to that which has been grown where the conditions of soil and climate are calculated to bring it to perfection, and that all inferior plants should be eliminated from the field before the harvest. It is highly important to have all the grains selected for seed purposes, uniform and perfectly matured. More important still is it to have the seed saved from a field where the plants are uniformly good, rather

than to select from a field where there are to be found many inferior, or below the type we wish to secure. By these means seed can be secured uniform in character and in period of maturity.

SELECTION.

There is not a field of grain, or of any kind of vegetable, but in which may be found some individual of superior merit. To give this an opportunity to grow is not only the work, but the duty of the plant breeder. It is the only road that will lead him to success; it is direct and certain. Encourage any superior growth; take it from its humble position and give it a higher one; minister to the necessities that growth, or development entails, and the results that follow will be proportionate to the efforts employed.

Time will not permit of our taking up this order of development in detail, but we cannot rest without calling your attention to an important field in which the seedsman or specialist has a golden opportunity. This field is the sugar beet, grown for seed purposes.

As before stated, the place to obtain seed is where a given type will, under good cultivation, reach the limit of possibilities. No vegetable should be grown for seed purposes anywhere else. While this rule is an important one in every line, it is doubly so with the sugar beet.

IS A CHANGE OF SEED NECESSARY?

Many persons consider a frequent change or removal of seed as an indispensible condition to the production of a profitable crop. The necessity of this change is insisted on by both theoretical and practical horticulturists, for reasons which they consider conclusive.

There is a very general impression that a change of "seed" is absolutely necessary for a good crop of potatoes, that if a given variety is grown for a succession of years on the same farm, deterioration, both in quality and quantity, will be the result. This opinion being held by neighbors, it is common practice for them to exchange stocks for planting. Than this, there can be no greater mistake, either in principle or practice. We know of farmers who usually get four hundred bushels per acre, or double an ordinary crop, who have not changed, and have used "seed" of their own saving for the past twenty years.

Under certain circumstances it is always best to get potatoes for seed purposes far from where they are to be planted for the production of a crop. This is particularly true when we have in view earliness of maturity, which, under many circumstances, is a matter of vital importance, as, for instance, where a second crop is to follow, as is the case on Long Island, where a second crop is the rule, rather than the exception, with our intensive system of farming. Seed grown at the extreme Northern point, where the variety will perfect its growth and perfectly mature its seed, will reproduce itself, in our latitude, several days earlier than that of our own production, which makes a second crop possible, where, if seed of our own production were used the result might not follow.

Our observation has proven most conclusively that it is not profitable to save "seed" from a crop grown from Northern "seed," as they are no earlier the second year, and not so productive. It is, therefore, much better to get seed from the North annually if a second crop is desirable.

ON ARTIFICIAL POLLINATION OF WHEAT

By William B. Alwood, Blacksburg, Va.

The writer began the study of varieties of wheat in 1882 upon taking charge of the field experiments of the Ohio Agricultural Experiment Station, and continued the work until the summer of 1886. Primarily the lines of investigation undertaken were purely practical, yet I trust enough of scientific importance was noted to warrant the presentation of these notes.

My attention was largely directed to grouping the cultivated varieties which could be obtained from home and foreign sources into several more or less clearly defined groups which have been published elsewhere (4th Rep. Ohio Agr'l Exp. Station, 1885).

The published accounts of work in artificial pollination of wheat which I had been able to obtain up to the time of beginning this work consisted merely of fragmentary notices in current publications devoted to agricultural matters, and up to the present time most that has appeared has been fragmentary and incomplete.

The writer worked two seasons—1882 and 1883—before learning how to manipulate the flowers so as to produce a cross with certainty. At first I worked with such implements as a poor dissecting case furnished, but soon confined myself to a couple pair of forceps and at last settled onto what proved the most successful instrument for this work that has come to my notice. This was an ordinary pair of No. 00 steel jeweler's forceps, with the ends dressed down to the size of a large pin for one and one-half inches back from the point, the point being left flat and one-sixteenth of an inch broad. This I found to be about the only instrument necessary, though others may be found convenient. My plan of operation was to select a head which suited the purpose in view, remove the spikelets from one side and those not desired on the other (usually leaving a couple more than I desired to operate upon).

It was generally found convenient to operate upon about five spikelets and of these the central flowering glumes were removed, leaving but the two outer ones. From these the anthers were then removed by taking the flowering glume between the thumb and forefinger of the left hand and carefully inserting the closed blades of the forceps between the glumes and palae, slowly releasing them until the parts are sufficiently opened to expose the enclosed anthers and stigma of the flower. By careful pressure of the thumb and finger of the left hand the flower is kept open and a dextrous operation with the forceps removes all three of the anthers at one operation. This completes the first part of the work. (The couple of spikelets left unworked are for observation as to maturity of the flowers. It is unsafe to allow them to

remain on the spike until they bloom, and after some experience one will get on quite well without them. The treated flowers were generally deprived of their anthers just as the spike made its appearance above the last leaf.

The second part of the operation is to fertilize the pistils of the treated flowers when ready to receive the pollen. The length of time between these two operations will vary according to maturity of flowers when the anthers were removed and also with conditions of weather. Successful crosses were made when both operations were performed on the same day and also when four days elapsed between the two operations.

Much of the success or failure of the experimenter depends upon the certainty with which he is able to furnish the ripening pistil with good healthy pollen. On this point the writer made an observation while at work in the field during the season of 1884, which ever afterward solved the question of securing proper pollen. This observation was the actual blossoming of one of the flowers on a head under treatment. The glumes were seen to part with a slow but plainly visible motion, the palae also parting with them and exposing the enclosed anthers and pistil to view. The width of the opening was not measured but was judged to be between one- and two-sixteenths of an inch. The filaments which support the anthers rapidly elongated, thrusting the latter through the opening when they immediately turned downward and shed their pollen.

(Noting that the pollen was shed suddenly just as the anthers were thrust out of the parts enclosing the flower, it occurred to me that that was the time to take the pollen for the work. This was afterwards followed except that I learned how to hasten the process of ripening by plucking heads on which flowers were nearly mature, drawing back the outer glumes and palets on one side and exposing the anthers to hot sunshine. The pollen was gathered in a small dish and applied with a camel's hair pencil to pistils of the flowers previously prepared, opening the glumes as already described, by means of the small forceps. The glumes and pales of worked flowers open when the pistil is ripe just as though the anthers had not been removed, hence pollen may reach a castrated flower by natural agencies.)

I do not wish to pass the operation of blooming as observed in numerous instances during my later work without noticing it more particularly. The time occupied by the flower in opening is from one to two minutes, the anthers can be seen to immediately rise out from the opened flower and shed their pollen, and strictly speaking I consider the period of bloom lasts about five minutes, certainly does not exceed ten. As soon as I saw the copious amount of pollen poured out among the spikelets on the head the fact was at once impressed upon me that the wheat flower was not of necessity strictly self-fertilizing. Any other flower on the same or adjacent head which might be open at this time is very liable to receive pollen and thus at least its own pollen be assisted in the act of fertilization. With the question of natural cross-fertilization in view I was led to examine more carefully the flowers and found that in a great many instances the filaments of the two-branched plumose stigma extend out laterally, far enough to reach quite through the enclosing parts, and in my estimation making it possible for them to receive pollen from the outside if by

by any chance it should not come to them through the opening of the flower. I attempted to follow up this matter and will refer to it again at the close of these notes.

As to the actual results of the work in artificial pollination I cannot say as much as I should like. The crosses made in 1884 were all destroyed by the severe winter of 1884-5, as was also nearly the whole experimental work with wheat, including some two hundred so-called varieties. With several varieties, a few stalks of which survived, the work was again taken up in '85 and the progeny of some of these crosses harvested in '86 are shown on the cards herewith exhibited. In all the crosses here shown, Velvet Chaff, which is truly a characteristic variety, was used as the male parent. This variety proved to be one of the most hardy with us and was the standard variety used in all the general experimental work. The specimens here exhibited show what a remarkable influence it had upon the three clean chaffed, beardless varieties with which it was crossed. These latter were Russian May, Siberian and Big Frame.

Referring again to the matter of whether ovaries are strictly fertilized with their own pollen in nature, I will say that a portion of the blossoms on a large number of heads were deprived of the anthers and left to note development of the kernel. In a great many of these kernels were formed and these afterward planted brought plants true to the type of the variety. If it is asked why were they not crosses, the answer would be that the heads of their own variety being so plentiful about them and the others being some distance removed, there was no opportunity for crossing.

Some of the heads produced in this experiment were eleven inches long by actual measurement and were well filled, but in the subsequent operations of the Ohio Station, after I severed my connection with it, these crosses were entirely lost.

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The Horticultural Society of New York

Incorporated 1902

MEMOIRS, VOLUME II

Proceedings of the

International Conference on Plant Hardiness and Acclimatization

1907

Held in the rooms of the American Institute of the City of New York, and in the Museum Building of the New York Botanical Garden

October 1st to 3rd, 1907

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Incorporated 1902

1907

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Papers Presented at the Conference

First Session, Tuesday, October 1

Second Session, Thursday October 3

Papers Read by Title

VIII

INTERNATIONAL CONFERENCE ON PLANT HARDINESS AND ACCLIMATIZATION.

First Session—Morning.

Held in the Rooms of the American Institute, 19 West 44th Street, New York City, October 1st, 1907, at 10 A. M., the President, James Wood, presiding.

The President—We meet in these rooms as the guests of the American Institute of the City of New York, and we have the President of the Institute here with us this morning, Mr. Rutter, and I am sure we will be pleased if Mr. Rutter would give us a word of welcome here.

Mr. Rutter—Mr. President and Gentlemen and Ladies: I am glad to see you, the members of the Horticultural Society of New York.

On behalf of the American Institute, it affords me very great pleasure to welcome you, not only to the City of New York, but to the use of the rooms of the American Institute.

In former years it held very large expositions, and every individual who had anything useful or anything novel that he wanted to introduce to the world, was only too ready to take space and exhibit it in the interests of the world, in these institutes, shows and exhibitions. Since that time, of course, the City of New York has changed to that extent that exhibitions of that kind at the present day would in no circumstances, in my judgment, prove a success, for every manufactory, every department store, every florist's establishment, has exhibitions of its own, that the public can see day by day, and in this way no interests would be served by such an undertaking by the American Institute at the present time, in the form of an exhibition, or an exposition.

I am pleased to say that the American Institute always takes an interest, and is always ready to afford its accommodations to kindred organizations, to all the societies that have at heart the welfare of the agriculturists.

I believe it has become an undisputed fact that not only the wealth of individuals, but the wealth of nations is derived from the soil, and the man or the woman who possesses the ability to make two blades of grass grow where only one grew years ago, has certainly proved a benefit to the world at large, and that is what you are doing, bringing to bear at the present time all the scientific knowledge, and all the scientific ap-

pliances to produce greater results from the soil than have ever been produced heretofore.

I am sure if you keep on, with the progress that has already been made, and advance still farther, which doubtless you will do, that we who are confined day by day and year by year within the bounds of a narrow office or manufactory, will only be too glad to take advantage of the benefits you have conferred upon us, to get into the open and enjoy the sun and the light of heaven.

I welcome you, gentlemen, in the house of the American Institute, trusting your deliberations will be pleasant and beneficial, and that you will leave us with a good impression of the City of New York.

The President—As to the particular subject of our meeting, we are here to do what we are because of the work of two great laws. One is the force of heredity and the other is the force of environment.

Under those two laws we are what we are, and those laws are of such wide and universal application that the subject to be considered comes under their operation.

Plants are influenced by heredity, and this force is potent, very potent. It is, however, subject to some changes and modifications, such as Professor DeVries has recently called our attention to.

Plants are influenced by their environment. Their environment and the environment of their progenitors have determined their character, and it is the change that comes upon their character by the influence of environment that we are to consider.

There are few more interesting things than the consideration of the application of the great universal laws, and I am quite sure that our meeting here and the papers which we will hear read, and which will be discussed, I hope, and I believe, will influence and enlarge our knowledge upon these interesting and important matters. The programme calls first for a paper from Dr. MacDougal, now of Tucson, Arizona, on "Factors Affecting the Seasonal Activities of Plants."

Before Dr. MacDougal begins the reading of his paper, I want to say that the new work he is introducing in Arizona is one for which he is eminently qualified. Of course, we know of his admirable work here in our own New York Botanical Garden, and that he is working there under most exceptional conditions, having a range of elevation of over a mile as his experiment station. He has the climate, he has it practically in his hand, and he has opportunities for observation that are equalled by few in the world.

Dr. MacDougal—Mr. President and Members: I think you have handicapped me by these preliminary remarks, because I shall not probably be able to show results equal to what you will expect.

The following paper was read by D. T. MacDougal:

Factors Affecting the Seasonal Activities of Plants.

By Dr. D. T. MacDougal,

The Desert Laboratory, Tucson, Arizona.

DISTRIBUTION AND ACCLIMATIZATION.

Every species inhabits the areas which it has been able to reach and occupy from the starting point of its place of origin. Neither its birthplace nor any of the places within its range may offer the most suitable conditions for the best growth and highest development. Beyond seas, over mountain ranges, across the equator or past other equally effective barriers may lie plains, valleys, plateaus or even continents, where if once introduced, it might overbear all competition from the plants already there, extending its distribution a thousand-fold and the number of its individuals a million-fold. Let the barriers be but once passed and it enters into a new kingdom, as the various invasions of weeds amply testify.

The soil, the various factors of climate, the course of the seasons, and the actual composition of the plant-covering already present in the region, may be such that the intruder becomes an integral part of the flora, and it may indeed perish in its original habitat and in the places successively occupied by it, leaving us no clew as to its place of origin.

The value of a cultivated plant is fairly co-ordinate with the extent of its possible distribution and culture. Not only does its greater cultivation bring a greater total product, but the greater the crop the better developed may become the facilities by which it and all of its constituents are used to the fullest, and to the greatest economy by the human race.

Our conscious efforts to widen the range of distribution and extent of cultivation of species of interest and economic value facilitates and aids one of the most basal processes in the life of the plant, and it has before it the possibilities of limitless suc-

cess, to compensate for the numerous failures which the worker must necessarily encounter.

Two main considerations confront us in the problems of acclimatization. First, a careful examination reveals the fact that nearly every species in the wider usage of the term includes several races or elementary species, which bear different hereditary qualities as to hardiness, capability for accommodation, rapidity of growth and productiveness. Careful culture enables a comparison to be made among a group of such forms and to select those which bear the desired qualities to make an introduction or acclimatization operation successful. Perhaps the necessary qualities may be discerned in separate races of elementary species, and by hybridizations these qualities are brought together into races or species capable of meeting the conditions to be encountered. To recount, or even adequately illustrate the triumphs and accomplishments of the horticulturist by methods for the most part very crude, during the last century, would be impossible.

Now, however, by the light of present knowledge, profiting by the splendid results of Nilsson with cereals, all such operations may be carried out with much greater exactitude and much more rapidly than by the old-time method of trial and error, most wasteful of skilled energy and time-consuming in human life. To-day we may expect as much from the breeder in ten years as he might have been able to accomplish in the previous half century. The realization has been tardily reached that if we are to make alterations in the hereditary qualities of the plants useful to us, we must make an accurate analysis to disclose the constituents of the species with which we are dealing, and having this information we may proceed with the exactitude of the chemist making compounds and extractions in his laboratory.

With so much to our credit in the way of advance made in knowledge of the nature of the plant and its behavior, the other important task which confronts us is that of making a similarly exact study of climate and of all of the factors which constitute the complex set of conditions which we term environment.

A simple analysis of the relations of a plant to external

conditions will be useful for a better understanding of the character of the problems involved. The principal factors affecting vegetation are undoubtedly light, temperature, moisture, food-material and chemical composition and physical consistency of the soil. It is obvious to the veriest novice in gardening that certain intensities or concentrations of these agencies are necessary for the welfare of the plant, and that the combinations suitable for one are not for another.

It will be impossible to give even a brief consideration of the special relations of each of these factors to the plant, but we may gain an insight into their general character by a consideration of the more important details with respect to temperature, which is one of the most widely interlocking elements of climate. The conclusions derived from its consideration may be held to apply to the other agencies as well.

Living matter is an extremely complex substance and we must be prepared therefore to find that its relation to its environment is not simple; this is especially marked with regard to temperature.

CARDINAL POINTS IN TEMPERATURE.

All of us know by every-day experience that there is a certain general degree of heat at which any given species grows best, and a discrimination as to the application and regulation of temperature constitutes one of the most important features of the practice of greenhouse gardening. This temperature, which is customarily termed the *optimum,* may be ascertained to within a degree or two very easily. If the heat is increased in a greenhouse in which a plant is happily growing at the *optimum,* it will soon be found that such increase lessens the rate and amount of growth, and a continued increase will soon bring the thermometer to a point where a *supra-optimum* will be reached at which growth ceases. This may simply bring the plant to rest as might the cold of autumn, and with but slight damage. But if the heat be increased still further a third point will be found at which the plant is killed and by such a test we will have ascertained the point of *fatal heat.*

Starting again with a plant at the *optimum* it will be found

that as the temperature decreases, growth slows down until an *infra-optimum* is reached at which growth ceases as it did at a certain point above; this is the temperature of *fatal cold* at which living matter is totally disorganized.

Our efforts at acclimatization and our work in securing the feature of hardiness in plants, with respect to temperature, consist in operations by which the position of the cardinal points of the plant with which we may be working may be altered on the scale of the thermometer. These cardinal points undergo wide changes in a state of nature, and it is by taking the inherited capacity for adaptation of any plant with regard to this particular into account that we may hope to make our greatest progress. First of all it is obvious that these five critical points in the life of any plant change with the development of the individual, and that the *optimum* slides up or down the scale, or all open out more widely. Take any plant, such as the radish, wheat, squash or sunflower, and it has been found that seed or grains air-dry, and in resting condition, may endure the lowest cold that can be produced, that of liquid hydrogen at about 454° F. below the freezing point of water, which proves that the fatal cold in such cases is extremely low, and to have only a theoretical existence. The same seeds in a resting and dried condition may be subjected to the heat of boiling water at 212° F., so that the points of fatal heat and cold lie far apart in this stage of the existence of the plant. Now give them a supply of moisture and start germination, and a radical change in the position and relation of the critical points ensues. A cold fatal to the active seedling will be found near the freezing point of water, and but slightly below the *infra-optimum,* the *optimum* will be found to lie between 80° and 98° F., the *supra-optimum* and cessation of growth will be found between 100° and 120° F. for most plants, although many species, especially those native to the desert, range higher, while a fatal heat comes within a few degrees above the *supra-optimum.*

As the plant nears maturity, the tissues harden, the protoplasm becomes more highly granulose and denser, and has an altered chemical composition, by which it again becomes less susceptible to alterations, and again the cardinal points take posi-

tions more widely separated from each other, and in the seed are again able to endure any cold which they may encounter.

THERMAL REQUIREMENTS OF A PLANT.

This brings us at once to the consideration of the practicability of some estimation of the thermal constant of any form, or the amount of heat necessary for its seasonal or cyclical development. The first effort toward fixing any such standard appears to have been made by Reaumur, the inventor of the thermometric scale which bears his name. He adopted the sum of the mean daily temperatures, as recorded by his thermometer in the shade, as an index of the amount of heat required to bring a plant to any given stage of development, using averages of the daily maximum and minimum to obtain his mean daily temperatures. According to Abbe, Reaumur calculated the sum of the averages constituting the heat exposure of a plant at his locality in France during the 91 days of April, May and June, 1734, to be equivalent to 1160° C., but in the following year it amounted to only 1015° C.

Adanson disregarded all temperatures below freezing, taking only the sum of the positive temperatures on the centigrade scale, and began the summation of heat exposures thus derived with the first day of the calendar year for any given season.

Boussingault attempted to derive the thermal constant of a vegetative period, or any part of it, by multiplying the mean temperature of the air by the duration in days.

Gasparin calculated thermal constants from temperatures obtained from insolation thermometers exposed to direct sunlight while lying on the sod, which would record 20° to 30° C. higher than shade temperatures, and showing a difference equivalent to three to six degrees latitude. By this method, the thermal constant required for the germination and maturity of the seed of wheat amounted to 2450° C.

Variations in the method of calculation of the thermal constant have been made by various investigators, in which this standard has been obtained by multiplying the mean temperature by the square of the number of days involved, others multiplying of days by the square of the mean daily temperatures.

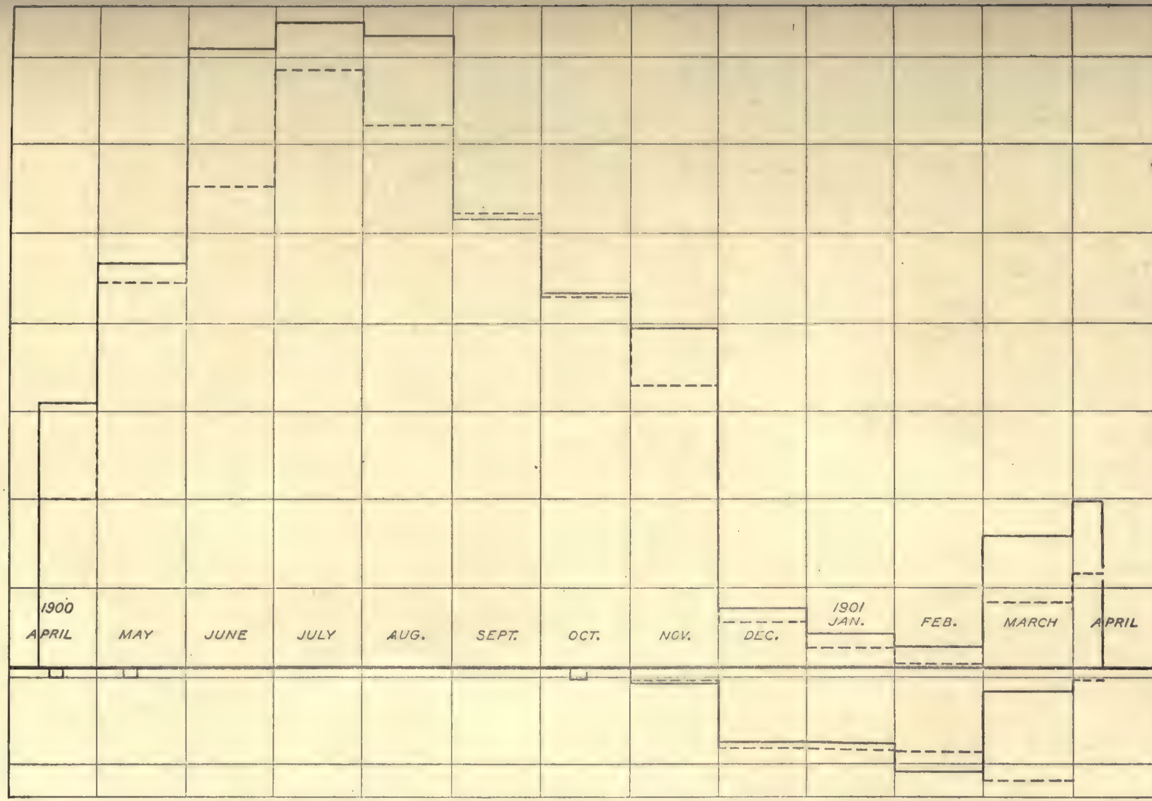
Some begin this calculation of the heat exposure with the appearance of the earliest species to show sign of awakening activity.

As an application of the principle of the thermal constant many bio-geographers have attempted to explain distribution by the mean annual temperature to the regions concerned. Among the most notable of such works is to be mentioned that of Hoffman, of Giessen, South Germany, who used the sum of the insolation temperatures from the 1st of January in calculating the thermal constants, and it is his data which are quoted so freely in all general treatises on plant geography and the thermal relations of vegetation. Drude uses the mean annual temperatures in his treatment of the subject, in which he is followed by Pound and Clement in their Phytogeography of Nebraska.

It need scarce be said that the mass of data accumulated by the various methods described during the last century and a half is confusing on account of the highly empirical character of the principles upon which each separate investigation has been based, the different standards of thermometry, and the utter lack of uniformity of technique. The last defect alone is sufficient to invalidate most of the results, which are nearly all valueless so far as any application is concerned, in this connection. Concerning the futility of research upon this subject it is most significant that Warming and Schimper refuse to recognize the thermal constant as a definable factor in the relations of plants to climate.

In the effort to outline some method for the calibration of heat exposure of plants growing in the open, the work of Herve Mangon seems to offer the most valuable suggestions. Mangon computes all shade temperatures from the time of germination of seeds until maturity of the plant was reached, disregarding all measurements in which the mean daily temperatures is less than 6° C. (42° F.). By this method he found the sum of mean daily temperatures necessary for the ripening of wheat in Normandy in 1870-1879 to vary between 2219 and 2517, and with the data of several seasons at hand it was possible to predict the date of ripening of wheat within three or four days.

The great variation shown by a plant with regard to the heat exposure calculated by Mangon's method is in all probabil-



Total heat exposure in New York Botanical Garden, April, 1900, to April, 1901. The areas above the heavy base line enclosed by the solid lines represent the total heat exposures in hour-degrees-centigrade, of the herbaceous grounds. The areas above the base line limited by the dotted lines, denote the exposure in the hemlock grove. Exposures below the freezing point in both localities are similarly represented below the base line.

ity due to the faulty method of calculating such exposures. The performances of an engine are not to be calculated by the total averages of the steam pressure during its working days, but may be quite exactly determined by multiplying the pressure by the number of hours during which pressure was kept up and used.

A similar relation holds with regard to the use and effect of radiant energy in the plant, and although any method of estimation must be more or less arbitrary, yet it seems possible to select one which will be capable of wide application and corresponding value. In the evolution of such a method for plants in the temperate zone it seems less artificial to begin the calculation of the heat exposure with the winter solstice instead of January 1st, and as has been done by several writers, or if economic plants are under consideration, take the date of planting as a starting point. It also seems most convenient to use the temperature of the freezing point of water as a base line for the thermometry of the plant.

The application of the method then simply entails the calculation of the number of hours to which a plant has been exposed to temperatures above the freezing point from the winter solstice or other starting point until the stage of development, such as flowering or fruiting, under consideration has been reached. The time factor is then properly applied to the height of the thermometer above the freezing point during the period mentioned. In actual practice this may be easily done by computing the area enclosed by a thermographic tracing of the temperature and the base line of the sheet for the period over which the development of a plant is to be studied by means of a planimeter. It was found by this method that the flowers of *Acer saccharinum* were mature and ready for fertilization on March 26, 1901, in the New York Botanical Garden, after 1100 hours' exposure to temperature above zero with a totality of 3109 hour-centigrade units: *Draba verna* attained the same stage something earlier in 974 hours, with 1644 hour-centigrade-units' exposure.

Now, it is by no means to be assumed that the above data represent the fixed and invariable constant heat exposure of the plants in question, for as has been described previously, the car-

dinal points, including the *optimum* for growth and development, may be altered by other conditions which affect the plant. The variation of which a plant is capable represents its possible geographical range which may be mapped with fair accuracy. Thus the individuals of a species which live nearest the pole have made such accommodations that they are able to accomplish development with a minimum number of heat units, in a minimum number of hours. As the range of a species is traversed toward the equator or to lower elevations, a place is reached where the heat exposures are at an *optimum* for the plant, and beyond this, development is retarded until the southern limit of the species is reached. The actual limits are of course determined by the entire complex of conditions, of which insolation is also an important factor, but for the sake of clearness, attention is focussed upon temperature alone.

The gradual accommodation and acclimatization of grains to regions to the northward has nowhere been more systematically studied than in the Scandinavian peninsula, and Schubeler's consideration of available results led to the conclusion that corn from lower latitudes or elevations ripened earlier when taken northward and upward, although the light and average temperature was less. This precocity in development persisted for some time, when seeds were taken back to the southern localities. In some cases the seeds and sometimes the leaves reached a greater size if the conditions permitted full development in the northern extensions, but this accommodation was not carried to the first generation in plants in the south from northern grown seeds. It was also noted that the colors of various organs as well as the aroma was increased in plants taken northward if the introduction did not go beyond the limit of conditions allowing full development. (Schubeler, F. C. Viridarum Norvegicum. Norges Växtrage. Et Bidrag til Nord-Europas Natur-Og Kulturhistorie. 1. Christiania, 1885. Rev. in Bot. centralb. 28: 203. 1886.)

The relation of the plant to negative exposures is one of endurance and not of performance, and the interpretation of the influence of cold upon distribution may not be made by the formula given above. The total amount of negative or cold exposure is undoubtedly the predominating one, but the minimum,

the range in variation, and the occurrence of minima below the freezing point during the vegetative season, are also of importance in distribution and await the acquisition of additional data before their interpretation may be attempted successfully. Some of these factors are extremely localized, and the poleward limit of distribution in the northern hemisphere of many species is known to run in extremely irregular lines.

Some illustration of this is gained from the results of the comparative study of the climate in the hemlock grove of the New York Botanical Garden and the meadow of the herbaceous grounds, not more than 500 yards distant, made in 1900 and 1901. The data obtained show that the meadow carpet received 78836 hour-degrees of heat during the year ending April 1, 1901, extending over 7282 hours of exposure to temperatures above the freezing point, while the hemlocks received but 68596 similar units with an exposure to temperatures above the freezing point for 7024 hours. The meadow was exposed to 5569 units of temperature below the freezing point and the hemlocks 5791 units. The herbaceous grounds were below the freezing point for 1478 hours, and the hemlocks for 1736 hours.

Here, then, in two localities within rifle shot are to be found two habitats for plants in which the difference in the length of the season as indicated by the number of hours above the freezing point amounts to nearly eleven days, the total number of heat units in the meadow being 13% in excess of that of the forest. The maximum and average maximum of the meadow are higher, and the minima and average minima are lower, the mean average of the hemlocks being lower, however, than that of the meadow. The value of such observations is greatly enhanced by the fact that they represent a comparatively narrow range of variation. Thus, in the several years in which observations were made as to the matter, the length of the period between the last frost of spring and the first of autumn lay between 167 and 171 days in the New York Botanical Garden.

The data upon which these conclusions rest are shown in the accompanying tables. The amounts given under "total exposure" represent the product of the number of degrees above freezing point multiplied by the number of hours. The value of the

centigrade-hour-unit represented in such amounts is 9/1600, and of Fahrenheit-hour-units 81/8000. The number of hour units is therefore to be found by simple division. + indicates temperature exposures above freezing; — below freezing.

THERMOGRAPHIC OBSERVATIONS IN N. Y. BOT. GARDEN, 1900-01.

	HERBACEOUS GROUNDS.		HEMLOCK GROVE.	
Date	Total Exposure	Number of Hours	Total Exposure	Number of Hours
April 9 to 16, 1900	+4.25	149	+3.50	152
	— .14	19	— .08	16
April 16 to 23	+9.50	168	+8.47	168
April 23 to 30	+8.45	168	+7.56	168
April 30 to May 7	+7.75	168	+8.50	168
May 7 to 14	+9.45	164	+10.25	168
	— .02	4		
May 14 to 21	11.56	168	10.91	168
May 21 to 28	12.37	168	11.76	168
May 28 to June 4	12.50	168	11.66	168
June 4 to 11	12.85	168	11.77	168
June 11 to 18	13.00	168	12.10	168
June 18 to 25	15.21	169	12.93	169
June 25 to July 2	16.32	167	14.17	167
July 2 to 9	16.85	168	15.16	168
July 9 to 16	15.80	168	15.42	168
July 16 to 23	17.73	168	15.19	168
July 23 to 30	15.35	168	14.42	168
July 30 to Aug. 6	14.46	168	13.00	168
Aug. 6 to 13	18.86	168	15.94	168
Aug. 13 to 20	15.66	168	14.10	168
Aug. 20 to 27	15.27	168	14.29	168
Aug. 27 to Sept. 3	16.36	168	15.17	168
Sept. 3 to 10	15.84	168	14.93	168
Sept. 10 to 17	13.25	168	13.24	168
Sept. 17 to 24	10.21	168	9.81	168
Sept. 24 to Oct. 1	12.35	168	10.12	168
Oct. 1 to 8	13.30	168	11.80	168
Oct. 8 to 15	9.30	168	8.56	168
Oct. 15 to 22	+6.25	159	+5.95	168
	—0.15	9	....	
Oct. 22 to 29	+10.25	168	+9.90	168
Oct. 29 to Nov. 5	7.16	168	7.70	168
Nov. 5 to 12	+4.30	156	+4.50	168
	— .24	12	....	
Nov. 12 to 19	+4.40	138	+2.90	135
	— .41	30	— .50	33
Nov. 19 to 26	+7.50	168	+6.05	168
Nov. 26 to Dec. 3	+2.86	146	+2.80	155
	— .65	22	— .30	13
Dec. 3 to 10	+3.15	133	+1.95	138
	— .71	35	— .50	30
Dec. 10 to 17	+ .60	32	+ .35	22
	—3.60	136	—5.90	146

Date	HERBACEOUS GROUNDS. Total Exposure	Number of Hours	HEMLOCK GROVE. Total Exposure	Number of Hours
Dec. 17 to 24, 1900	+1.50	74	+1.27	48
	—2.25	94	+1.28	120
Dec. 24 to 31	+1.50	90	+ .90	65
	— .85	78	— .90	103
Dec. 31 to Jan. 7, 1901	+ .80	68	+ .50	40
	—2.10	100	+2.84	128
Jan. 7 to 14	+ .80	140	+ .60	126
	— .60	28	— .40	42
Jan. 14 to 21	+1.10	68	+ .65	46
	—2.60	100	—2.70	122
Jan. 21 to 28	+ .80	102	+ .50	42
	— .60	66	—1.00	126
Jan. 28 to Feb. 4	+2.25	20	+ ...	...
	—4.00	148	—3.55	168
Feb. 4 to 11	+ ...	...	...	...
	—4.30	—168	—4.80	168
Feb. 11 to 18	+ .50	— 56	+ .20	32
	—2.20	—112	—2.10	136
Feb. 18 to 25	+ .50	36	+ .20	14
	—1.50	—132	—3.00	—154
Feb. 25 to Mch. 4	+1.50	88	+1.20	76
	—1.20	— 80	—1.50	— 92
Mch. 4 to 11	+1.80	102	+1.20	108
	—1.70	— 56	—1.65	— 60
Mch. 11 to 18	+2.40	149	+1.40	145
	— .30	— 19	— .20	— 23
Mch. 18 to 25	+3.60	154	+2.70	149
	— .10	14	— .20	19
Mch. 25 to April 1	+7.56	162	+2.65	141
	— .10	— 6	— .25	— 27
April 1 to 8	4.70	168	3.41	168

STIMULATION AND ACCOMMODATION.

There yet remains to be considered the stimulative reactions and accommodations of the plant under changes in the environmental forces which act upon it. Generally speaking, it may be said that sudden changes in the intensity with which a force acts upon a plant results in stimulation, and that gradual alterations are followed by accommodations, and that such adjustments or adaptations may be produced by the long-continued uniform action of any external factor.

A striking example of stimulation followed by accommodation is offered by the sensitive plant, and the well-known response of this plant to a touch or blow consists in folding movements of its leaves and leaflets. In repeating the test of it, perhaps this blow may be given by a drop of falling water, or by a

slender pencil. Now place a healthy plant under a fine shower nozzle from which water not too cold will fall in thousands of repetitions upon the same leaves. The first of the mimic shower causes the leaves to undergo the characteristic movements. The steady, gentle tapping of the falling drops continues, however, and the leaves become so accustomed or accommodated to their shock that they no longer constitute a stimulus, with the result that in a few hours the leaves are fully expanded in the falling drops, the first touch of which caused a full and rapid closure of all of the leaves and leaflets. The accommodation is to falling drops only, since if the leaves are struck with a rod, or exposed to the action of heat from a shielded burning match, closure follows. The test may be repeated by arranging a clockwork to cause a small rod to strike the leaves or stems at frequent intervals, when accommodation will follow in the same manner. This is one phase of accommodation. A second is one in which a force is slowly increased. Thus, suppose that instead of suddenly exposing the plant to a shower of drops, we had placed it in a damp chamber and began spraying it from an atomizer in which the size of the particles of water was slowly increased until they became large raindrops. Treatment of this kind would be followed by no reaction movement, the protoplasm having ample opportunity to make the necessary adjustment to the size of the drops and the increased force of the blows.

An even much more familiar illustration of stimulation is that offered by the practice of storing dormant bulbs and tubers at a low temperature, then bringing them into a warmer room for sprouting. The change in the temperature is the shock which awakens the resting plant in such operations, and the difference between the storage pit and the growing chamber may be so great that it should be made in two steps to avoid damage. On the other hand the beneficial effects of such stimulation may be readily appreciated when plants are stored at temperatures too high to secure this shock by the change, and it also accounts in part for the slow, feeble action of some species when kept at an equable temperature during the entire year. But this stimulus is not to be thought of as always a change from a low to a high temperature, for the reverse may have a like effect. *Encelia*

farinosa is a desert shrub which has been introduced into the xero-montane plantation of the Desert Laboratory at 6,000 feet, being taken from a habitat at 2,500 feet and correspondingly warmer. It is a winter perennial, however, and its season of activity lies within the cool season of February to April, at which time it goes into a resting condition. Now, if the roots are taken up at this time and carried up to the 6,000-foot level, the stimulus of change to the cooler temperature again causes it to awaken and put out a new set of shoots. Exact observations upon this stimulative reaction of any plant are possible, and many of the practices of the gardener are the results of long practical experience upon the matter. An interesting set of data have recently been obtained by Dr. B. E. Livingston with respect to the change from the *infra-optimum* to the *optimum* with regard to moisture from which it is seen that seeds of *Cereus, Fouquieria, Phaseolus* and *Triticum,* germinated when transferred from an air-dry condition to soil containing 15% of water, *Impatiens* in soil containing 20 to 25%, *Raphanus* demanded 20%, and *Trifolium* 25%.

Gradual changes in the temperature, or in any of the other agencies affecting plants, may allow the protoplasm to make such adjustments of the living matter that the cardinal points are much changed and a species may accommodate itself to conditions ordinarily unendurable. Experimentation in this phase of the subject has been carried on most with bacteria and the simpler fungi, and it has been found that these organisms are capable of making such slow changes as to be able to undergo temperatures, comparatively very low and very high. The facility with which these organisms may be handled has also led to the result that they have been found to accommodate themselves to very great changes in the composition of the nutrient medium, and to endure the presence of poisonous substances, the concentration of which was increased very gradually.

In all such cases the power of endurance of the plant to an excessive or defective action of any one force is very much modified by the presence of or action of others. Thus, in the matter of the seeds, the endurance to extremes of temperatures is seen to depend directly upon the amount of moisture present.

Again the time element or the rapidity with which the in-

tensity of external conditions is changed is a basal factor in all accommodation processes. As a plant accommodates itself to live at unwonted temperatures, or in unaccustomed soils, but little doubt exists that it undergoes changes in intricate structure, which, however, are not always to be demonstrated. So long as the species remains under the new and strange conditions the acquired structure will be retained from generation to generation, whether propagated by cuttings or seeds. If the species is returned to its original habitat or to the normal conditions in which it originally grew, the acquired structures may persist for a time, but in accordance with the power of accommodation which originally brought them into existence, they will disappear when the inducing factors are removed.

The classical cultures of Bonnier made in the Alps and Pyrenees, twenty years ago, furnish us with the bulk of the systematic information available as to the influence of elevation on plants. From these it was seen that plants taken from lower to higher altitudes, up to about 7,000 feet, and not exceeding the *optimum* of the species, developed shorter internodes, the subterranean parts of the plant were relatively much larger, the leaves were smaller, more deeply colored, and the flowers were also more vividly tinted.

No better illustration of the changes in structure shown by plants, when accommodating themselves to habitats presenting strange external conditions, can be found than those found in the American water-cress (*Roripa Americana*), with which some extensive experimentation has been carried on. Originally taken from the muddy bottom of Lake Champlain, where it grows in water at a depth of 1 to 3 feet, it had been gradually accommodated to the propagating bed, the cold frame, and the hothouse in the New York Botanical Garden, from whence it has been successfully transferred to the Laboratory at Cinchona, Jamaica, and to the montane plantation of the Desert Laboratory in the Santa Catalina Mountains in Arizona. During this dissemination it has substituted radish-like structures for the bunch of fibrous roots characteristic of it, and developed new forms of leaves and stems, while throughout it shows tissues and arrangements of tissues wholly unfamiliar to it; all of which has been brought

Location of xero-montane plantation of Desert Laboratory among piñons and junipers, at 6,100 feet, on southern slope of Santa Catalina Mountains, Arizona.

about with comparative rapidity in five years. On the other hand, *Lysimachia terrestris* has been transferred from a terrestrial habitat to an aquatic, with similar sweeping changes by way of accommodation in even a briefer period.

So important are the possible results in this phase of experimentation held to be that the Department of Botanical Research of the Carnegie Institution of Washington has established plantations, under permits from the management of the National Forest, at 8,000 feet, in a moist alpine climate, and at 6,000 feet in an arid situation in the Santa Catalina Mountains, in connection with a small experimental farm, at 2,200 feet in the alluvial irrigated soil of the Santa Cruz Valley near Tucson. Without going into detail at this time it may be stated in general that the experimental work carried on at these plantations involves an interchange of plants among the three stations, and also introductions from various locations in different parts of the world. In the two seasons that have elapsed since organized, ample reward has been obtained for the effort expended.

The methods and the results discussed above refer wholly to adaptive or responsive changes made by the bodies of plants subjected to any given environment, and forming accommodations to it. These alterations are of the greatest importance in the extension of the range of any plant, and by a study of them the accommodation response may excite the plant to increase the very feature of its structure of economic importance, and suppress those useless or harmful in its application to our needs.

Still a last possibility is to be taken into account in the great changes to which plants are subjected in acclimatization work. I have recently demonstrated that external agencies may be made to act upon the germ cells of plants in such manner that changes take place which are expressed in the progeny, and are heritable from generation to generation, constituting in fact the origin of new forms having the standing of species. The experimental methods used are fairly simulated by natural forces Likewise Dr. Tower has been able to induce similar changes in the germ cells of beetles by the application of such stimuli as variations in temperature and moisture. It is to be said, therefore, that in taking plants from their native habitats to the

Montane plantation of Desert Laboratory among spruces and poplars, at 8,000 feet, on a northern slope, Santa Catalina Mountains, Arizona.

uttermost extent of their possible ranges, we possibly subject them to forces which may be a most potent factor in the origination of new qualities and new lines of heredity.

GENERAL CONSIDERATIONS.

A more direct application of the ideas elaborated in the foregoing may be possible by a brief restatement of the more important generalizations.

The forces or factors affecting vegetation are simple physical properties capable of ready apprehension and easy measurement. Much is known as to the mode of response, reaction, accommodation and adaptation to such single factors or to a complex of them, and further experimentation upon the problems involved may be readily organized.

The thermal requirements of two plants, as indicated by the records of a season, have been tested, and with reasonable allowance for variation may be taken as characteristic of the forms in question. The method used consists in the measurement of the number of hour-degrees exposure, beginning with the winter solstice or with the germination of seeds.

The difference between localities only a few hundred feet apart at the same altitude may be sufficiently large to make existence in one impossible to a form which may find its *optimum* in the other.

The stimulative reactions of plants to sudden changes in environmental conditions form the basis of many important gardening practices. On the other hand the capacity of the plant for accommodation to conditions widely different from the average, following gradual changes, is very great and is perhaps the most important phase of the subject with respect to acclimatization. The more extensive studies in this problem have been concerned with the northward extension of the cultivation of fruits and cereals, and comparative cultures at low and high altitudes.

It is to be recalled in closing that but few plants occupy more than a fraction of all of the possible habitats by non-conscious distributional movements, and that the intelligent consideration of the factors of climate and a development of cultural methods may most readily secure the economic dissemination of

plants from the localities in which they do grow to the full range of habitats where they may grow. Not only may species be carried and established in numberless new localities offering conditions equivalent to their natural habitats, but a study of the adjustments and accommodations of which the plant is capable enables or allows it to be introduced into unfamiliar conditions, under which the structural response may take on qualities more valuable than those usually shown by it.

The President—Dr. MacDougal has given us in that portion of his paper which he has read, very valuable points, and the paper and the subject is now before the meeting for discussion; I am sure that Dr. MacDougal would be glad to answer any questions that may be asked him upon any point that has been presented.

Does anyone desire to make any remark upon this subject just presented by Dr. MacDougal?

A. L. Willis—What is the range, Dr. MacDougal?

Dr. MacDougal—It covers the common prickly pear of this region —and then we have the Indian turnip and the columbine and several others that are taken from Northern Lake Champlain and the climate in the Botanical Garden—which has now had its range extended until this plant is growing in the desert laboratory, at 6,100 feet, and also in the soil at 8,000 feet.

I will take the liberty of saying that this produced the widest adaptation which has yet been recorded concerning any plant.

Mr. Southwick—I would like to ask the widest range of temperature, the widest variation?

Dr. MacDougal—We have the walnut trees on the top and on the bottom, looking very different. I don't know whether they really range up and down the whole mountain or not. Some one has submitted specimens of these walnut trees to Dr. Britton, and he thinks they are inseparable. Now, here is a vertical range of a mile. I do not believe the juniper would cover much over a vertical mile. I know some other plants which will cover this. I think there is one which has a range greater than this. This is a plant that grows at sea level in Mexico, on the west coast, in latitude about 27, and then it goes northward seven or eight hundred miles and climbs up to four thousand feet. I believe that is the widest range of anything I know of.

On the wideness of range, I don't believe any study has yet been made of the botanical features or capacity on which this wideness of range rests. I don't know what there is about the juniper that makes it do this, or the walnut tree, either.

Mr. Southwick—How about the birches?

Dr. MacDougal—The birches are too much mixed up, sir, for me to follow them. I don't want to venture anything on the birches.

Mr. Southwick—I would like to ask you another question. Do you believe there are some ripening processes? May it not be the case, Dr. MacDougal, that there may be a ripening process in the seed which is to be fulfilled before the seed will germinate? You know that we find that the Solanum tuberosum has to have a ripening process before it will germinate? In other words, it will not germinate until spring. Is it not possible that the seed will have some ripening processes, changing sugar into starch and starch into sugar before it will germinate?

Dr. MacDougal—This is a very elusive subject. In my paper, I say that these seeds were made to germinate seven or eight months ahead of the time when they normally would germinate. That is to say, Professor Southwick, the seeds are made to germinate when they would not ordinarily until next February, so if there is a ripening process there, it is finished very quickly. That matter of the ripening period is something I have never been able to get any information on that is at all satisfactory.

Mr. Southwick—Is it true about Solanum tuberosum?

Dr. MacDougal—It is true that some will germinate in September and some will not until later. That whole matter is a very elusive one, and I have never been able to make a very satisfactory statement of it, to myself, or anybody else.

Dr. Evans — In some experiments with the nightshades, I have been able to germinate the seed at once in the fall, and raise them during the winter in the greenhouse. The peculiar thing about it was that the plants were set all winter and would not do anything in the way of growth until about the normal time in the spring. A most remarkable case this was. I was never able to force them. They simply stood dormant after the seed had made a few leaves, and with the gooseberry and currant (the wild species) I found several cases where they would not force during the winter. If the buds were just about open, they would stay until about the normal time in the spring. Last fall up in Lapland where I went for the Department of Agriculture, I noticed a most remarkable situation in the experiments going on at Lulea. That is up in the extreme northern part of Sweden, the Gulf of Bothnia. They have an experiment station there for the purpose of hardening plants, making them more resistant to cold. Two ice houses were constructed, one a cooling house and one a freezing house. They had them constructed so they could hold the temperature to a fraction of a degree, if necessary. They had some barley and some cereals in boxes in the earth, and at different times they were submitted first to a cooling process and then to a freezing process, imitating untimely freezing in the spring and fall. The trouble in that province is that barley especially being the main cereal up there, was apt to freeze out about once in 11 years. I found them laboriously selecting the plant that had grown under

those extraordinary conditions, to be frozen in the midst of a full vegetative growth, and I suppose the idea was to force the germination.

I am just wondering whether the reverse could not be done: To put the plant into an extremely hot climate for a time to see if they could not force the plant to adapt itself to a hotter place.

Mr. Macoun—We have growing at the experiment farm now, quite a number of Solanum tuberosum. It is ripening in 62 or 63 degrees in Canada. It seems to me a very interesting fact, because it shows the wide range of the potato. We usually consider the potato, so far as the ripening of the seed is concerned, as having a very wide range, but this, as you know, is a very good many miles north of your latitude, 62, and I think my father was up there in '72 and he noticed the great abundance of potatoes the next spring, after wintering over in the ground, and I feel certain that the seed must have remained over, instead of the tubers, because the seed germinated with a very high percentage of vitality, and I think the point might be very interesting in this discussion.

Dr. Hansen—I was much surprised last fall to see the potato cultivated a good way north of the Arctic Circle in Europe. The potato has come from Archangel on the Arctic Ocean, down to the experiment station in Northern Norway, and up nearly to the Circle, and they were raising it in that locality. They were rather small, but they matured in that latitude. That shows there has been a change in its early maturity.

Mr. Evans—In connection with our experiment up in Alaska, I want to say just a word. We have a regular experiment station, of which you will probably hear later on. But speaking of the potato; there is a station up there which is about 60 miles north of the Arctic Circle, and the man there sent me last summer two potatoes that weighed nearly ¾ of a lb. each, well matured, and as a curiosity he saved a number of seed balls with fully developed seeds, and they were sent early in the season, in August, but they were not yet ripe. Up in Alaska, the potato has been grown for the last three years to my knowledge successfully as a garden vegetable.

The President—It has been a surprise to me to see the extreme northerly latitude in which the potato grows. I am quite sure I have seen it a hundred miles north of the Arctic Circle. I don't know about the maturing of the seeds but the tubers matured sufficiently to grow from the eyes without any apparent difficulty. Indeed, they were not able to tell me when the varieties they were growing were introduced into that region, showing for a long period they had continued to grow from the tuber. I was not able to get any information in regard to the seed. I would state also, though it has no specific value, that the potatoes grown there are of very excellent quality.

We will now hear a paper on "Air Drainage as Affecting the Acclimatization of Plants," by Ernst A. Bessey, Subtropical Laboratory, Miami, Florida.

The following paper was read by Ernst A. Bessey:

Air Drainage as Affecting the Acclimatization of Plants.

By Ernst A. Bessey,

Pathologist in Charge of the Subtropical Laboratory, and Garden, Miami, Florida.

To those living in Florida is offered a most excellent opportunity to see extensive experiments in acclimatization and to note some of the factors influencing their success or failure. Besides the numerous importations in recent years by the United States Department of Agriculture, there have been ever since the settlement of the State, innumerable attempts to establish plants from all parts of the world, from both temperate and tropical regions. It was to be expected that only a small percentage of these attempts would result in success on account of the many factors coming into play. For instance, the question of soil has probably accounted for more failures than any other factor except minimum temperature. In almost the entire State of Florida the soil is to a very large extent composed of sand, in many places very deep and with no subsoil. In these sandy soils the large annual rainfall (50 to 60 inches) is counterbalanced by the porosity of the soil, which often causes the death of a plant from lack of water, while in the heavier soils of California the same plant may thrive with a far smaller annual precipitation. The lack of plant food in this sand must also be taken into consideration, as it is of great importance, it being impossible to replace it satisfactorily in all cases by the use of fertilizers.

Another very important factor is the fact that in Florida the chief period of rainfall is in the warmer months. This is distinctly advantageous in most cases, being in direct contrast to the conditions in California, where the rainy season is in winter.

The most important factor of all, however, is the question of temperature, especially that of the minimum winter temperature. Two classes of plants can be distinguished here, viz., those that are killed by a more or less long-continued period of cool nights which do not reach the freezing temperature, and those which endure such periods without injury but which are killed or seriously injured by a comparatively slight and short drop in temperature below the freezing point. It is of certain factors affecting the acclimatization of this latter group that I wish to speak.

It has been long recognized that the slope of the land has a great bearing upon the liability to injury of fruit trees from late spring frosts. So, for example, where danger of frost injury to peaches is to be feared, the orchards are planted on hill slopes or near steep inclines which will drain away the cold air. During the winter this air drainage is of no avail since the whole mass of air is cold. In the spring and fall, however, when the general mass of air is warm the air next to the ground is cooled by radiation, forming a cold layer of limited depth.

In southern Florida, e. g., in the vicinity of Miami, the temperature of the mass of air never falls low enough to cause death of plants by freezing. By radiation, however, the layers of air next to the ground sometimes become colder than freezing temperature. It is under such conditions that air drainage becomes a factor in acclimatization. If the general air mass should occasionally become cooled below freezing temperature, those plants mentioned above which are subject to injury by such temperature would be killed out, regardless of location, but since, at Miami, the cold air is only of limited depth the location determines whether the plant is killed or not.

The principles stated above were clearly demonstrated at Miami last winter (1906). During the daytime of December 23d, 24th and 25th, there was a heavy, cool northwest wind, but the sun shone brightly and the temperature remained about 50°F. At sunset each of these days the wind ceased almost absolutely, and the temperature began to fall at once. On the morning of the 24th, at the Subtropical Laboratory building about seven feet from the ground, the temperature was 32° F.; on the 25th,

28°, and on the 26th, 26°. Immediately after sunrise the temperature rose at once to above freezing. This lowering of temperature was not due to the down-flow of cold air from above but solely to radiation from the soil and other objects. This was demonstrated by the fact that a lath roof which could not have prevented the inflow of cold air from above, for the spaces between the laths were the width of the laths themselves, prevented radiation to such a degree that pineapples under this roof suffered no injury, while where the roof was lacking, owing to previous destruction by wind, the plants were badly injured, although no lateral barrier was between the plants.

A few days after the freeze I drove all around the country about Miami making observations upon the effects of the cold. It was at once apparent that the layer of air causing the damage was not deep. In few places did any injury appear more than four feet above the ground, while in many spots the injury was confined to within a foot or two. At the Subtropical Garden a number of two-year-old mango and avocado trees were killed. For several days, in one case for several weeks, the tops remained green, then suddenly died. I found that for a distance of six inches to a foot from the ground the bark and young wood had been killed. The same observation was made on a number of tamarind, india rubber and other trees set out along the avenue for ornamental purposes.

In several places the injury to the foliage of tender plants was noticeable from the ground up to a certain level, the line of demarcation between killed and uninjured foliage being very distinct and perfectly level. In one place, especially, where a grove of avocado trees stood in a kind of basin about one hundred yards across, the injury was not apparent to the trees at the margin but reached higher and higher the further one advanced into the center of the basin, while at the other side the injury again became less and less. Sighting across it was clear that a lake of cold air, level at the top, had occupied this basin.

In marked contrast to such places as this was the absolute lack of injury where the slope of the surface was such as to permit the cold air to flow off. Thus at Cocoanut Grove, about four miles south of Miami, the land near the bay is sloping

while half a mile or so inland it becomes level. On this level portion the injury from cold was very severe, but on the sloping portion it was entirely wanting except where obstacles intervened to hold back the cold air. So, for example, one rather steep slope showed no signs of injury whatever, except near the base, where a close stone wall about five feet high held back the cold air, acting as a dam and forming, as it were, a little pond. The plants standing in this pond of cold air were seriously injured. That the general lack of injury was not due to the proximity of the bay is shown by the fact that at the Subtropical Laboratory and Garden, right by the water, but level and with the outflow of cold air hindered by a fence and trees, the injury was exceedingly severe.

To summarize: air drainage is an important factor in the acclimatization of plants in those regions where the general mass of air is not cold but the cold occurs in a rather shallow layer of air. Under such conditions with proper drainage the cold air flows off, preventing injury.

The President—Miami is an extremely interesting place for a study of this kind, and I think there is real value in the paper that has been presented. Does any one wish to make any remarks upon it?

Dr. MacDougal—This is an interesting paper. Dr. Bessey brings to mind some very accurate experiments made in the same place some fourteen or fifteen years ago by Dr. Roberts at the time of the great destruction of citrus fruits. These experiments were published in 1893. This is a subject that I have been interested in and have published a little on, the last being in 1899, in which I find this inversion of temperature a very important factor in the distribution of wild plants. I find it reaches its greatest accentuation in regions of low humidity and not in arid regions. I have published some observations in a valley where it drains from a mountain range 13,000 feet high. Now, a thermograph at this place, and the observations and the result show the minimum record at times often varies as much as fifteen or twenty degrees, so it may be twenty degrees below freezing and up on the hill a few hundred feet above, it may be twenty degrees warmer. Now, these inversions of temperature have the effect of carrying the distribution of wild plants southward, that is to say, downward, down the mountain slopes, in these ravines where the air is cold at night and the plant runs down where it makes existence possible there. This air drainage is accountable for a great many so-called anomalies. That is, you may have a plant which belongs on the top of a mountain, and find it down some ravine where the cold air flows.

Mr. Macoun—I would like to give some observations as to the death of plants in cold weather.

In the Province of Behring in Canada, and also in the United States, I believe one of the chief causes of death in fruit trees is the killing of the trunks in winter, which we believe is due to the constant flow of cold air during the winter months. We had a very good example of it in Ottawa, which as you know is not in Behring Province, last year, when we had a kind of winter killing which we have never had before in twenty years' experience. I believe this coincides in a very large extent, with the winter in Behring Province. We had a very serious kind of winter, with greater cold than usual. There had been a steady flow of air all through the winter; that is much more than the average. The result was a large number of apple trees which were killed in the trunk for about two feet entirely around the trunk. Now, as a rule, the form of winter killing in the winter that is known as sun killing usually occurs on the south and southwest side of the tree, and is due largely, we believe, to the sudden changes of temperature, thawing and freezing, but in these cases, I suppose, there were one hundred or one hundred and fifty trees destroyed in that way, killing right around the trunk. The bark and the trunk were destroyed. The trees leaved out as usual. Some are living yet, but they gradually die and those that did not die last year will die next year. I just wanted to give this example and give one in Behring Province, where the fruit trees are killed by too much air drainage.

Mr. Southwick—I think it is a well-known fact that the lowering of the temperature of the soil has the same effect upon plants in appearance at least as frost. You take the eucalyptus plant. If you have a very wet cold soil the plants die. That is the result of my experiment in Central Park now. They were planted near the house in the shade and the moisture and the lowering of the temperature has caused them all to die and I think it is a well-known fact and a very important fact, every act of the lowering of the temperature and the excessive moisture deprives the plant's leaves of the nourishment they require, and they die. I think Dr. MacDougal can say a few words about that.

Dr. MacDougal—It is true in regard to the eucalyptus plant that the fatal cold in some places is between 36 and 38 degrees Fahrenheit. That is to say, they freeze to death before the freezing point is reached. They are not killed by cold, so that this thing you speak of is probably a combination.

Mr. Macoun—I would just like to say, as I mentioned before, that we believe—although I haven't any data to prove it—that the driest part of a tree is the trunk. Perhaps Dr. Hansen has some information on that point. We know that near the tip there is more moisture than any other part, but I believe the driest part of a tree is the trunk and this constant flow of air in the winter dries out the trunk so that there is

not sufficient moisture to stand the strain, and the result is that the tree dies from lack of moisture, and the bark is killed by the lack of moisture.

T. V. Munson—I think the name of air drainage, in some places is not quite the right name. We have in connection with drainage a down flow in my region, located on the south side of Red River, located some one hundred and seventy to two hundred feet above the bed of the river. I have known several times that we have succeeded splendidly with certain varieties of fruit, peaches, for example, while those on the opposite side of the river, the north side of the river, Red River running in that region from west to east, that we had full crops while on the other side they had none. There seemed on those occasions, a very slow movement of the atmosphere from north to south, what we would term a "freezing norther." It seems that in that case the valley of the river is quite broad and heavily forested, so that the heat that had accumulated in the valley during the day gradually flowed upward over the bluff on the south side of the river and kept the trees at such a temperature that they were saved, while on the north side they were lost, but in other cases where we have had still freezes, that is, where the air became apparently a dead freeze, great destruction was the result. I have seen that illustrated in my own old place, north of Denison. The elevation of the hill was about fifty feet above that of the little creek where I reside, and frequently plants were killed in the bottom, the same varieties that were not hurt at the top of the hill, and I observed that the difference in temperature was between 8 and 10 degrees. Now, in that case, and in some other cases, the drainage seems to be a wedge of cold air settling gradually down into the hollow, and displacing the warm air there, and it flows up over the hill. That is the character of the drainage, it seems to me. I remember very distinctly, four or five years ago quite a severe wet freeze came when the grapes had pushed from 6 to 8 inch shoots, nearly at the blooming period, and my present place lay pretty well on the little plateau between Red River and the Creek south, and on the upper portion of that grapes were not damaged at all. We made a good crop. I noticed at the lower portion of our place, probably fifty foot below the upper portion, frost began and from there on for fifty miles, all vineyards in the valley were killed. There were no grapes at all, except on the scattered secondary buds pushed forward, and in this case it would seem that there was a lake of cold air below the freezing temperature—to be considered a lake, although it may have had a slow motion down the valley and creek. The creek is quite crooked and pretty well forested, so there would be very little motion, but in that regard, it was like a lake, like the lake mentioned —and below the line there was no damage.

The President—I would like to mention one or two illustrations, of the economic influences and values resulting from this question of this fact of air drainage.

In the north of this latitude, the peach crop is somewhat uncertain, as a rule, taking it over the country generally. It is a fact in this whole

country of northern New York, that when a peach orchard is planted on a slope of a hill where there is active air drainage, the crop is much more reliable. Then there is another thing in connection with this that if, in this cold north country of ours, the peach orchard is planted on the northern slope of a hill, it is vastly more reliable than when planted on the southern slope of a hill, if it has active air drainage. Of course, there is another problem involved in this, and all these problems are very complex, gentlemen. We must not draw our conclusions from insufficient data, because Nature's problems are so complex. The northern slope is preferable to the southern, because there is greater uniformity of temperature during the late winter and early spring, and with the southern exposure, the direct rays of the sun to the square foot are very great, and you have the soil thawing out when the temperature rises, whereas on the same hill, an eighth of a mile away on the northern slope, there is no change perceptible whatever. There is no slope and the sun's rays impinging on the square foot are not more than one-quarter, and indeed, hardly as much. Of course, if the hill is steep enough, there is no impinging of the rays at all, and the only heat you get is the diffused heat of the atmosphere. Now, in regard to this air drainage, there are great results from it. The greatest fruit country in the United States, and probably in North America, with the exception of the citrus counties in Florida, is Ulster County, just north of us. Peaches were not grown there thirty or forty years ago because they were utterly unreliable. They were planted in sheltered valleys where the northerly winds would not strike them. They supposed that the home of the peach was in Persia in a warm climate, and so they thought the peach trees should be planted in a warm valley. The fruit culturists in Ulster County were advised to plant their peach orchards on the northern slopes. The result was that the finest peaches in the city market to-day are the peaches grown in that county. If you go down to the great Washington Market of this city where the fruit is received, and you see any specially fine fruit to-day, and ask where it comes from, the chances are it is Ulster County, but it may be Northern New York, but it is grown on the hillside where there is the most complete air drainage, and on the northern slope, because the fluctuations of temperature are much less there than on the southern slope.

You know the native hemlock is a northern tree. I have often said that it is the most beautiful evergreen on this earth. I have never seen anything to qualify me in that statement, that the hemlock is the most beautiful evergreen to be found. It is a northern tree. It is said that the most southern hemlock grove is the one which is growing in Bronx Park, but you know the hemlock is found far south on the Carolinas—perhaps in Georgia as well. I don't know as to that. It may be in Georgia around on the mountain ranges. I happened to be interested in an extensive tract of land in Virginia, including a considerable portion, some forty or fifty thousand acres, on the Shenandoah Mountain.

We are lumbering to-day the northern hemlock on the slopes of the Shenandoah Mountain, where they exist, because of the air drainage, in my opinion, from the mountain range, coming down the slopes, as it is easy to see by the configuration of the mountain that the air comes down. Hemlocks are found away down the valley, where but for this air drainage, I believe there would not be a hemlock within a hundred miles, and here they are, magnificent great specimens, with trunks six feet in diameter, and making marketable timber 80 or 100 feet up the trunk. They are there, and the only way you can account for their being there is the air drainage from the mountain coming down into the warmer valleys bringing with it the possibilities of a vegetation of another latitude altogether.

Mr. Southwick—I think the factor omitted in what you just said is this:

I was brought up in Greene County, New York, and raised peaches there and farmed it until I was twenty years old. I found by planting our peach orchards on the north side of the hill, they were retarded in their growth in spring, and to add to that, we tamped the snow around the trees and kept them back a while longer, and we were free from having the buds frozen, and from them we had some beautiful peaches. We kept the ground there cold and in this way the trees were kept back. I think that is a very important factor.

Dr. Hansen—I would like to quote an experiment. I do not think that the last speaker did keep back the buds of a tree. I would not like to have that point passed without stating that the result of experiment has shown that such a process will not keep back the blossoms of any tree.

Mr. Macoun—We have tried many careful experiments of tamping the snow about the trees, and it does not keep back the buds at all.

The President—I will supplement my remarks by saying I have a peach orchard about 35 miles north of the city on a slope of a hill perhaps nine hundred feet above the level of the sea, and that orchard has not failed in giving a most satisfactory crop in fifteen years. That illustrates two things: first, it illustrates the vigor of the tree, that the peach orchard should continue for fifteen years in a good condition. Secondly, it also illustrates the principle I have been speaking of, that it is usually uniform; whereas, in the whole region around in the valley, they have not had in this fifteen years this peach orchard has been fruiting, without any exception—they have not had a peach crop in more than a third of the time. I would say one thing further in regard to this particular locality, an elevation of 900 feet, a northern slope where the sun does not strike in winter, and where there is perfect air drainage. In my boyhood I used to go up to this point, and there was the greatest peach tree I had ever seen. Its trunk was 22 inches in diameter and I found by consulting the old people, that the age of that tree was over seventy years. A peach tree in this latitude seventy years old, with a trunk 22 inches in diameter! The property was not then in possession of my

family, but the old owner of the land used to say that he would go up there and he would think that perhaps there would be a bushel of peaches on that tree and he would pick them and find ten bushels on one peach tree. I do not think it was the soil. I think it was simply the situation that caused the whole result.

Mr. Munson—Just one word in order that the example I propose may not be misconstrued. The slope of the Gulf from the north side extends through several miles very gradually up and down over small hills and valleys, and on the southern side likewise, so that the killing occurred entirely over the territory north for several miles, while for several miles each side of the river, we had abundant peach orchards. This could not be explained by the southern exposure of hillsides, and must be accounted for by some other theory, and the only theory that they could possibly think of was the slope to the southern side, and the heat in the valley that gradually drifted southward in that case. Now, as to drainage, the idea has been advanced that the drainage about a mountain comes from the top down the side of a mountain. I am inclined to take issue with that view. The flow is not steadily downward next to the earth from a mountain. It is upward. There is a high swift current of air passing over the top of a mountain. If you will observe over any ridge over which the wind flows in snow-time, the wind blows over the top, and you will find a counter-current coming up and you will find a hollow place. The current comes over it and hits the ground beyond and banks the snow up and there will be a hollow behind it. So it is with the circulation about a mountain; the air passes over it, the warm air from below comes up the opposite side of the mountain, and that cold air takes its place beyond, and you will find that the freezing is heaviest and begins first in the centre of the valley on a still night. This wedge of cold air from above settles down, and the warm air follows the hillside and flows upward. You build a fire somewhere near a large tree, and you will find that the flames, instead of going up, will seek the side of a tree and will go up the tree, and so the hill has the same power of furnishing a sort of back for the air to creep up while the cold currents drop beyond.

Mr. Macoun—I think that Mr. Munson is confusing two things, the movement of winds and the gravity falling of cold air. When we speak of air drainage, and the inversion of temperature, we consider the air resting on a plane as absolutely still. Therefore, what he says about air currents across the top of mountains has nothing to do with it. When air gets cooler it drops, and if you cool it on the top of a mountain, it of course, flows down the slopes. In my work on the Rocky Mountains, I had always found that if there are any mosquitos, it is advantageous to camp in one of these cold-air spots, as it drives the mosquitos away, and so our camp is always at the mouth of a cañon, leading up to a mountain, knowing it will not be salubrious for the mosquitos. This matter of inversion of temperature is a matter that has been studied by thermometers

and instruments of all kinds, and I am quite astonished to have this conclusion of Mr. Munson's quite contrary to our observations, and I think his statement is due to the fact that he is confusing wind action with the actual settling of cold air.

The President—I think that the difference in the two opinions comes from these facts: Mr. Munson comes from Texas. The cold weather they have there comes from the north. They do not have any fall, so far as I have found out, in Texas, although I have not been there the year around. Our temperature here falls the most when there is no wind, and that is where you have the air drainage. Buds of our fruit trees are killed on still nights, not on windy nights. They are killed when it is a perfectly still night, when there is no movement of atmosphere. I think here you have the explanation of the difference. I have had experience in the Gulf and in Mexico, myself.

Mr. Munson—Just a question to Dr. MacDougal: In driving across the plains in Nebraska and the West, hundreds of times I have experienced this, that we would come to a ravine that would be several miles in length, having a very little down flow, and beyond as we began driving down into that ravine, we felt a body of cold air, and felt quite chilly, while it was quite comfortable on top. On very still evenings when we drive through it, we feel those conditions. How do you explain that?

Dr. MacDougal—I think it is a splendid explanation of my theory, and I am glad to get it. Just imagine you had a fine mist of rain which is heavier than air, poured on the ground. It naturally seeks the lowest places, doesn't it?

Mr. Munson—I would like to know where cold air comes from?

Dr. MacDougal—It is cooled all over the surface of the ground and flows to the lowest places.

The President—We do not think of cold air coming from high elevations and coming down somewhere. The effect of cold air comes from radiation all over, and it runs like water, and as that water that falls upon the ground runs down into the ground, so this little stream of cold air runs down the valley.

Dr. MacDougal—It is more accentuated, of course.

Mr. Munson—Our thermometer all along the surface of the ground and up on the hill does not show that temperature which you should get on the bottom.

Dr. MacDougal—There is not enough of it there. Just as soon as it is cooled, it begins to move. The moment you have a particle of air that is colder than any particle next to it, it begins to move, so you have on this hill slope particles of cool air that begin to move down into the hollow. You do not have a lake on the side of a hill; you have a lake falling and running down in a thin sheet.

The President—If your thermometer was on a slope a foot above the ground, it would not be in the stream of cold air at all. If your

thermometer was exactly on the flowing line, it would give it, but really there is not enough of it. There would be radiation enough from the ground so perhaps you would not get any.

Mr. Munson—Has any downflowing current been discovered near the ground?

Dr. MacDougal—I don't know how we are going to escape it. It has been a matter of observation with me. I am using the mountain in my illustration, because here we have a thing that is most accentuated. Take this cañon situation. The wind there becomes so strong as to become actually unpleasant, and in the autumn or in the spring, much of the time we find the temperature is too uncertain for comfort. I find the Indian, and next to the Indian, the prospector, and the man who is out of doors, avoids these cañons and keeps on the cooler ridges where he is not bothered at all. This movement is reversed during the day. You have the upflow during the day and the downflow during the night.

Dr. Gager—May I say a word in response to a request for a specific instance of the observation of such an occurrence? The matter has been very thoroughly worked out on Cayuga Lake, by the observations of Cornell University, and the result has been published as to this downflow of cold air.

A recess was here taken until two in the afternoon.

First Session—Afternoon.

The President—The time for our Afternoon Session has arrived. We will have a paper on "The Real Factors in Acclimatization," by Frederic E. Clements, University of Nebraska. In the absence of Mr. Clements, Dr. MacDougal will read the paper.

The following paper, by Frederic E. Clements, was read by D. T. MacDougal:

The Real Factors in Acclimatization.

By Frederic E. Clements,
Department of Botany, University of Michigan.

The exact study of plant environments by means of instruments, which has been carried on in the Colorado mountains for the past ten years, has thrown light upon a number of current ideas as to acclimatization and hardiness. It is hoped that a brief statement of some of the results obtained in nature may prove of interest and value to the scientific horticulturist actually engaged in acclimatizing plants.

The advantages of a mountain region for studies of acclimatization are decisive. On Pike's Peak, for example, as many climates are found in ten miles as occur between the fortieth and seventy-fifth parallels, a distance of 3,000 miles. The climatic zones of the mountains are further broken up into a great number of local climates, owing to the extremely irregular surface and the varying maturity of the different land areas. The result is a diversity of climates not to be found elsewhere, and an accessibility to different climates that is unique.

In experimenting to determine the causes of the well-known dwarfing of alpine plants, it was found that the influence of light is negligible, and that water, in the form of soil water, is more important than temperature. Since dwarfing is merely the characteristic sign of alpine adjustment, i. e., acclimatization, it seems evident that for all dry regions at least, water, and not temperature, must be regarded as the controlling factor in acclimatization, and hence in hardiness and winter-killing. This conclusion has been repeatedly tested with the alpine shrubs and trees at timber line. On many of the Colorado peaks, as elsewhere, the timber line is fringed with a zone of thickets, chiefly willows and birches. These regularly reach their highest altitudes in moist depressions, especially where the exposure to bit-

ter cold and strong winds is extreme. In such cases, the drying-out at low temperatures, and not the actual cold, seems to be the cause of winter-killing. Additional evidence of this is furnished by the alpine spruces and pines. These extend 500 to 600 feet higher where they occur in shallow ravines and depressions with a higher water content, particularly on the exposed north and northwest slopes. This same phenomenon is even more marked on the Continental Divide, where the alpine thickets carry the spruces to outposts far above timber line proper by increasing the water content of the soil, decreasing the depth of frost, and protecting the young trees from the drying effects of the constant winds of winter. The upper timber line on mountains is accordingly a line of winter-killing. Its direct cause is not the extreme cold of winter, but the excessive water loss at a time when the water supply is chiefly in the form of ice, and hence non-available.

The feeling that the climatic habits of plants are fixed is widespread though it appears to rest upon no definite foundation. It has led not infrequently to the statement that woody plants, and trees in particular, cannot be acclimatized. The evidence from the mountain forests of Colorado is distinctly in favor of the conclusion that trees and other woody plants can be acclimatized. Indeed, it only requires reciprocal planting at various altitudes to constitute final proof. The spruces (*Picea engelmannii* and *P. parryana*), the firs (*Abies concolor* and *A. lasiocarpa*), the yellow pine (*Pinus scopulorum*), lodgepole pine (*P. murrayana*) and limber pine (*P. flexilis*), and even the Douglas spruce (*Pseudotsuga mucronata*), have repeatedly been found growing vigorously in such divergent local climates, measured in terms of water content, humidity, temperature and soil, that the conclusion is unavoidable that they adjust themselves readily to new climatic conditions. For a time the apparent distribution of the aspen well within the forest zone of the mountains was a puzzling exception, though an extra-regional outpost was known at 4,000 feet in western Nebraska. This difficulty was finally cleared up by finding that the aspen occasionally reaches the lower limit of *Pinus scopulorum* at the base of the foothills. It was also found this summer growing

at an altitude of 12,000 feet on Long's Peak, in the form of a dwarf shrub scarcely a foot high. That trees show great differences in their adjustability is too well-known for comment, but it has not been so clearly recognized that the restricted range of a native species may be due as much to a lack of migration as to the difficulty of adjustment to physical factors. The plumed pine (*Pinus aristata*) is in general restricted to the region of the upper timber line, and the piñon (*P. edulis*) to the lower. This distribution is primarily a matter of migration, shown by the fact that the plumed pine is represented by many outposts due to migration taking place more readily down the mountainside than upward.

The whole question of acclimatization hinges upon the meaning given to the term climate. In ordinary usage, climate means weather, and it is applied to conditions and areas of the vaguest limits both physically and geographically. General meteorological results which are chiefly restricted to temperature and rainfall are taken as the usual criteria, and factors of greater importance, water content, humidity and evaporation, largely ignored. The first step toward the exact study of acclimatization must rest upon a thorough investigation of the many factors that compose a climate. This can only be done by an extensive and intensive study by means of instruments.

By climate we usually understand the atmospheric factors of a region merely, and, as indicated above, soil factors are equally important, and often more important. For these reasons, the ecologist prefers to substitute the term habitat, as the sum of all factors that affect the plant, for climate, and to replace acclimatization by the word ecesis, the process of becoming established in a new home. These terms at least serve the purpose of emphasizing the fact that all of a plant's environment must be taken into account in studying its behavior and structure. Climate is too general as well as too restricted a concept for scientific purposes, at least for the biologist. Botanist and horticulturist alike speak of the climate of Colorado, of Nebraska, etc., when as a matter of fact the differences between sets of physical factors, or habitats, within each are often much greater than differences between neighboring States.

Colorado has a half-dozen or more distinct climates, while Nebraska, though more uniform, shows markedly different climates in prairie, sandhill and foothill region.

The use of instruments for measuring all the physical factors of the plant's environment is imperative if we are to know exactly what conditions the plant has to meet, and how it meets them. Furthermore, it is only in this way that we can discover real differences between habitats, or climates, determine the best source for promising forms, decide upon the most practicable method of treatment, and forecast the probable outcome. The exact measurement of the water of the soil, and the humidity of the air, of light, temperature, evaporation, wind, soil salts, exposure, slope, etc., will work a revolution in the methods and results of plant introduction and plant evolution. It will force attention to the fact that regions and areas which possess essentially the same climate, or weather, really show physical differences of the greatest importance to the experimenter. Finally, it seems probable that exact ecological methods of this kind will demonstrate that all variation is first or last only a question of environment; that the minute variations of plants, as Darwin understood them, can be traced to minute physical differences of the habitat, and that, as the final crown of the work, measured sets of physical factors and measured habitats can be made to produce definite desired adaptations in both cultivated and native plants.

The President—Cannot you supplement it with remarks, Dr. MacDougal?

Dr. MacDougal—I find much in this paper which is in accord with my own views. I rather object to anyone picking out any one physical factor and saying it is the keynote of the business, because it is not. He might as well say the left hind wheel of a wagon is most important, but it is not; on the other hand, the four wheels are equally important, and they get out of line unless all four are together. When Professor Clements says the soil moisture is the most important factor in a given situation, he is considering something to be a fact which is not a fact. In the main, however, I think I can say that I agree with him in the principal things.

Mr. H. A. Siebrecht—There is one thing I notice in this connection, and that is that the plant or tree suffers more from dryness or drought in cold weather. That I have experimented with myself, I find it is a fact, and I have often spoken of it in this way, to use a common every-day

phrase. I said a man with a full stomach can stand a good blast of wind in cold weather, whereas a person with an empty stomach cannot stand so much. I have found that out with plants. For instance, we take evergreens, planting them in vases in the fall of the year, taking up all the roots, and putting them into those tubs or vases, or places where they have not been growing, just for decorative purposes—I find that as long as the moisture is available, they stand all right. But as soon as it freezes up, and dry-cold wind comes, then the plant suffers and it sort of curls and folds up and it generally gets its death-blow during such a period, when we have the very extreme cold, and no water is available. I find that where they are exposed and where they can get the moisture, they can stand so much more cold, than where they are more dry, and I think that is very correct.

The President—I was struck, in listening to this paper, with the sweeping statement concerning moisture. I expected to see some proof of this statement, but he does not offer any. It is simply an assertion. What we want is the proof. Now, in regard to this question of moisture, if you look at the condition of various plants in their natural habitat, you will notice in the district north of this city where we have very rocky hills and our forests are very rocky, that you find trees growing in situations where they are as nearly destitute of water as they would be in any desert in Arizona, except the moisture of the air. There seems to be no soil moisture. I have given special attention to this matter of the growth of forest trees where the situation would seem to indicate, if not absolutely prove, an absence of soil moisture that would be accounted fatal to almost anything. Perhaps we may say that these roots penetrate to the crevices of the rocks to great depth. Indeed, they do undoubtedly, but owing to the situation, the supply of soil moisture is extremely limited, but still they will stand the droughts of summer and the freezing of winter and come through with marvelous hardiness. Of course, they have the same degree of air moisture as other trees. In fact, whoever is familiar with the forest region of our country notices that this rather conflicts with his assertion.

Dr. Hansen—Just an observation which ought to go on the record, possibly, in this connection: We find it a common practice in the northwest where we have 40 degrees below Fahrenheit, sometimes without any snow, that it is a great advantage not to permit apple trees or other fruit trees or plants to go into winter quarters without an abundance of moisture in the soil, to give the plants or trees a thorough watering just before they freeze up, otherwise they are very much more injured.

The President—There is a common expression all through the United States, to the effect that it is very disastrous when the winter sets in before the swamps are full of water. When the swamps are full of water, then let winter come, but before the swamps are full, it is disastrous, and occasionally, we will have a season that is so dry that the swamps are not full of water, and then we see in the plantings of our

lawns or in our forests, a great destruction of trees, greater than we ever find at any other time. That is in line with the Professor's assertion.

The President—Our next paper is "Evaporation as a Climatic Factor Influencing Vegetation," by Dr. B. E. Livingston, Tucson, Arizona. In Dr. Livingston's absence, we will have the paper read.

The following paper, by B. E. Livingston, was read:

Evaporation as a Climatic Factor Influencing Vegetation.

By Burton Edward Livingston,
Desert Botanical Laboratory, Tucson, Arizona.

All plants, excepting aquatics, are influenced in their growth and behavior to a marked degree by fluctuations in the external water supply. This is almost wholly due to the continual loss of water occurring from all aerial surfaces of the plant body, especially from those surfaces which, by the presentation of wet membranes to the air, are possible channels for the entrance of the food material, carbon dioxide, and for the exit of the waste product, oxygen. By far the greater portion of the water that the plant appears to use is not used at all in the true sense, but makes up for the loss by transpiration, and is itself soon lost by transpiration. This water must, of course, be drawn from the soil by the roots and transmitted to the transpiring organs by the conducting system.

The rate of water loss by any plant, while it does not strictly follow the rate of evaporation in its hourly fluctuations, is determined for the entire day or for longer periods by the evaporation rate, i. e., by the evaporating power of the air around it. This is of course true only so long as the water supply of the soil is adequate to keep the tissues at their normal condition of saturation.

Every plant has a certain maximum rate of water intake through its roots, this maximum being highest for any stage of growth with the *optimum* amount of water in the soil. This rate increases as the root system becomes more extensive, and decreases when for any reason the root system is injured or partially destroyed. Of course this maximum also decreases with a lowering of the moisture content of the soil.

There must likewise be a maximum rate at which water can

be transmitted through the conducting system to the leaves, etc., and this rate will probably decrease as the plant develops, since the path traversed becomes longer. We know practically nothing of the fluctuations of this latter rate, but it is probably safe to assume that it does not vary rapidly and is practically constant as long as it is not altered by growth.

Evidently, a resultant of the conditions just described is this, that the plant is able to maintain in its tissues a water content adequate to life and growth only so long as the transpiration rate does not exceed the maximum rate of water supply from the soil. It is thus possible for a plant to grow normally in a soil with a low water content, if only the evaporating power of the air about its leaves and branches (and hence its transpiration rate) is not excessive. The same plant may be observed to wilt and die at a later stage, for either, or both, of two very different causes: the water content of the soil may be decreased till the rate of supply to the transpiring tissues becomes less than that of loss, or the evaporating power of the air may be increased till the rate of loss becomes greater than that of supply. It will be seen that both causes bring about the same condition within the plant, namely, a shortage of water in the transpiring tissues.

From my own observations it appears to be true also, that a quiescent existence can be maintained with the rate of transpiration approaching or equalling the maximum rate of supply, but that growth cannot occur unless there is still a considerable margin of possible supply over and above the transpiration loss which is being experienced.

It is realized by every worker with plants that the water supply is the most important and fundamental of the conditions which are effective in determining growth. But it is not nearly so clearly appreciated that this is not alone dependent upon the amount of water in the soil but is to a great extent determined by the evaporating power of the air. At first thought it might appear that with high evaporation rate the soil would rapidly dry out and that at the same time the rainfall would be scanty, so that the air condition would be directly transmitted to the soil. But such is not the case, at least in many instances; for

with rapid evaporation the surface layers of the soil often dry out so rapidly that the moisture of the lower layers does not have time to diffuse upward to the surface before a functional dust mulch is formed, which almost completely checks evaporation. We are thus confronted with the seemingly paradoxical fact that a high evaporation rate acts to really conserve water in the deeper soil layers. Neither does the amount and distribution of the rainfall furnish evidence by which evaporation conditions may be surely predicted, for a time of heavy thunder-showers may show a great precipitation for a particular week when the evaporation rate was uniformly high throughout the period, excepting for a few hours before and after each shower.

Doubtless one reason for the neglect of evaporation as a climatic factor is that the evaporimeters which have been devised are unsatisfactory in one way or another, so that the weather services of the world have been able to do but little with this subject. In weather prediction relative humidity has come to be regarded as giving the same information as would rate of evaporation, but it leaves out of account the factor of wind, which is extremely important in determining the evaporation conditions. Numerous as have been the efforts of various workers to establish a formula by which the evaporation rate may be derived from the data of the ordinarily observed climatological elements, the suggested formulas are all as unsatisfactory, if not more so, than the different forms of evaporimeters which have been devised. These formulas differ markedly from one another, and the best, could one but select it, must be regarded as only approximate.

With a suitable instrument, evaporation is far better suited to the needs of the botanist and agriculturist than is relative humidity. In the first place, humidity variations affect the plant only through their effect on the evaporation rate, so that the evaporimeter gives direct information regarding the physical conditions to which the plant is subjected. In the second place, the evaporimeter is a self-integrating instrument, like the rain gauge, but unlike the thermometer or hygrometer; for any given period a single reading gives the sum of all the evaporation in-

crements which have occurred within that period. Thus an average rate is easily and quickly determined.

In attempting to determine some of the factors limiting the vegetation of the arid regions of southwestern America the writer has been led to a study of evaporation and its effects on plant life. The remark is often made in these regions that "with water, anything whatever could be made to grow here"; a statement which is far too broad. With water properly applied, it is certain that a large number of plants will succeed in the desert which could not otherwise live in such a climate; but all plants thus succeeding, unless it be in seasons of frequent rains when evaporation is retarded, must be adapted to withstand the high evaporating power of the air. A large number of the ordinary mesophilous plants of gardens are possessed of this adaptation, but others are not. The native desert plants usually have it to a great degree, but many of them are not so fortunate in regard to the opposite power, to withstand a wet soil and low evaporation rate at the same time.

A number of plants were tested in this regard during the summer just past, at the Desert Laboratory, Tucson, Arizona. Narrow beds were prepared in the open ground, being only five or six inches across and separated by irrigation trenches of the same width and of an equal depth. The soil was kept moist by lateral seepage from these trenches, which were filled once or twice a day. The water stood in the trenches only a few hours at a time and the soil did not at any time become water-logged. It was constantly very near its *optimum* water content. The soil was a heavy adobe clay, similar to that of the Chinese truck gardens near Tucson.

The evaporimeter used in recording the evaporating power of the air for this experiment was the porous cup form devised by the author and described in Publication No. 50 of the Carnegie Institution. The evaporating surface is provided by a cup of porous clay about five inches in length and three-quarters of an inch in diameter, closed at one end and reinforced by a thickened rim at the open end. The material of this cup is similar to that of the ordinary Chamberland filter tubes. The opening is closed by a perforated rubber stopper carrying a glass

tube about fifteen inches in length, the latter passing through a cork stopper into a Mason jar, the container for the supply of water. Cup and tube are filled with distilled water and the tube is carefully inserted into the jar, which has been previously filled, in such manner that no air is allowed to enter the tube. Evaporation takes place from the surface of the cup and water to make up for the loss thus occasioned is drawn from the jar below. The jar bears a file mark on its side to indicate the standard level, and is filled to this mark at each reading, the amount of distilled water required for this being measured and furnishing the reading of the instrument. The apparatus was placed with the jar beneath the soil surface, the cup projecting about six inches above. Readings were taken once a week, which was found to be often enough for the work in hand. Considerable trouble was experienced this season with a consignment of impure water, which so clogged the pores of the cups that the latter became useless. Fortunately for the work, an instrument had been installed at the University of Arizona, by Dr. W. B. MacCallum, and his records have been used in place of those from the instruments which were injured. The University campus is so situated that the evaporation conditions there are very similar to those of the plantation where this work was carried out.

Among the plants tested were: the garden nasturtium (*Tropaeolum*), morning-glory, marigold, sunflower (*Helianthus annuus*), mustard, castor bean, muskmelon, teasel (*Dipsacus sylvestris*), and jimson-weed (*Datura Stramonium*). The seeds were sown in May, in the midst of the spring dry season, and the behavior of the plants was watched till early September, when the experiment was brought to a close.

From the time of planting, the drought conditions continually increased in severity until July 6th, when the summer rainy season opened. From May 13th to July 1st the weekly average of the daily temperature maxima rose from 87 to 107° F., while the corresponding average of temperature minima rose from 45 to 75° F. For the same period the weekly average of maxima and minima of relative humidity decreased from 45% (maximum) and 32% (minimum) to 31% (maximum) and

17% (minimum). The average weekly evaporation rate for the period from May 13th to July 1st was, as given by Dr. MacCullum's data, 323.8 cc.

The seeds germinated somewhat tardily but otherwise in a perfectly normal manner. With the castor bean and musk-melon, after a few days in the cotyledon stage growth was rapid, more so in the former than in the latter; but growth became still more rapid in both cases after July 6th. The other forms of the series slowly developed a few leaves and then practically ceased to grow at all. Day after day they were examined without the detection of any difference from the condition of the preceding day. The plants did not wilt and appeared healthy, except for the lack of growth. There was *some* growth in all cases during this period but it was so small in amount that it was practically negligible.

After July 6th the weekly averages of . the daily maxima and minima of temperature and relative humidity were very close to those exhibited for the week ending August 26th, which were: for temperature, 100° and 80° F., and for relative humidity, 95% and 59%. It is at once seen that the temperature conditions were not greatly different during the season of summer rains from those of the spring dry season, so that the response in growth of the plants, which I am about to describe, cannot be related to this factor. The humidity conditions for the two periods were about what was observed in the case of the evaporation rate, which showed an average of 185 cc. per week during the second period, i. e., a decrease to about 57% of its rate during the dry season, as given previously (383.8 cc.). After the beginning of the rains irrigation was unnecessary excepting a few times when the showers were too far separated and the soil began to show signs of drying out. It is, of course, theoretically possible that the plant responses here to be described were caused by the change from well-water to rain-water. This is not at all probable on account of the fact that the saline content of the well-water is mainly calcium and magnesium carbonates, and that these exist in the soil in rather large amounts, so that the rain-water must almost immediately become of practically

the same concentration as well-water. The responses of the plants were immediate and no time elapsed for the leaching of soluble salts from the soil by the first rains; in fact, a change in the behavior of the plants was observed before any precipitation occurred, for the evaporating power of the air began to decrease several days before rain fell.

With the coming of the summer season the plants quite generally responded in a very marked degree. The castor bean and muskmelon increased their growth, which had been rapid before. Morning-glory, sunflower, marigold, mustard and jimson-weed, all came rapidly into flower and fruit in a perfectly normal manner, and this within a surprisingly short time. The small rosettes of teasel which had been developed showed almost no response. They are still alive at the time of writing, but are only about three-quarters of an inch across. Nasturtiums were also scarcely affected by the change in conditions; they finally succumbed during the rainy season. A single plant, which was situated so as to be in the shade of a creosote bush for a few hours of the day, lived several days longer than the others, suggesting that light intensity may possibly have had a part to play in the failure of this form to succeed. But it seems altogether more probable that the plants of this form had been so injured by the untoward conditions of the dry season that they were unable to recover. The latter idea is supported by an observation made several times previous to the present experiment, that nasturtiums failed to succeed at the Laboratory, when grown in pots which were kept well watered. I have never started the plants from the seed in the rainy season.

The meaning of this entire experiment may be expressed in this way: that castor bean and muskmelon were able to absorb and transmit water from the soil to their foliage considerably faster than it was lost by transpiration, and hence were able to carry on an active growth even during the intense drought. The garden nasturtium and teasel failed to provide the excess of water needed for good growth even in the rainy season (although the latter form did not fail as completely as did the former). The other forms of the experiment were unable to supply the needed excess of water in the dry season, but pro-

vided it in a normal way in the season of summer rains. The power to absorb and transmit water was sufficient in all the forms to support life (that is, to prevent wilting and desiccation) during the drought period, but was adequate for growth during this period only in castor bean and muskmelon.

That the responses just described were due to the change in the evaporating power of the air is hardly to be doubted. There was not a sufficiently great change in temperature to account for it, as has been shown in preceding paragraphs. It might be suggested that the decrease in light intensity incident to the oncoming of the summer season may have been the cause of the response noted; but in the absence of reliable data as to the effect of such variations in light intensity (we have as yet no practicable photometer for such studies that measures the energy of the light as a whole instead of measuring mainly the less refrangible portion) and in the face of the *a priori* consideration that it is fully as probable that such variations affect the plant through changes in the evaporating rate as that the light intensity is *per se* the active agent, the evaporation power of the air seems by far the most probable climatic element to which to attribute the plant responses here dealt with.

Some of the native desert perennials respond to the change in seasons in a marked way, as by losing their foliage in the dry season; but it is difficult in these cases to distinguish between the effects of dry soil and those of high evaporating power of the air. The deeper soil layers of the Desert Laboratory reservation contain throughout the year a considerable water supply, so that it is entirely possible that the more deeply rooting perennials do not suffer directly from desiccation of the soil. A case where the rate of evaporation appears to be the controlling factor in the seasonal response is furnished by the experiment of Lloyd with *Fouquieria splendens,* in which he was able to cause the local development of leaves during the dry season by simply wrapping a portion of the leafless stem with a cloth kept wet by means of a siphon. No water was allowed to reach the soil about the plant, so that the response must have been due either to the local check imposed upon transpiration or to water absorbed through the bark of the stem, bud scales, etc. From

what is known of the low rates of water absorption by stems and leaves it seems very improbable that the latter supposition is true. If the other is correct, it would appear that the *Fouquieria* plant in question was absorbing water and transmitting it up the stem at a rate which was inadequate for the development of foliage, as long as the evaporation (and transpiration) remained excessive, but that this rate became adequate for leaf formation with the local decrease in evaporation rate.

Another apparent instance of a response to evaporation conditions is afforded by *Sphaeralcea pedata,* a small red-flowered mallow attaining a height of about two feet, and bearing flowers and leaves the year round. In the winter and early spring (the season under the influence of the winter rains) the leaves have an area of from twenty to thirty times as great as in the spring dry season. While the leaves of the earlier season are bright green and nearly smooth, those formed in the season of drought are densely covered with white tomentose hairs. Growth is less rapid in the dry season and the longer branches often die back to the ground at this time. The leaves of the more mesophilous type mostly succumb before the arrival of the summer rains, leaving a plant of an appearance entirely different from that of the early spring. In the summer rainy season this plant resumes its more rapid growth and the leaves then produced are of the mesophilous type in the shade and partake of this character to a large extent even in the sunshine. It was observed that in the dry season potted plants of *Sphaeralcea* growing in a soil kept at the *optimum* water content refused to develop the mesophilous type of leaf, and that the same specimens produced that type during the season of low evaporation rate. The main difference noted between the potted plants and those in the open ground lay in the fact that the former retained the mesophilous leaves produced in the early spring somewhat longer than did the latter.

This evidence is of course very inconclusive, but it seems indicated at least that even with the soil kept at its *optimum* water content, *Sphaeralcea* responds to high evaporating power of the air by the assumption of an entirely different form and

structure of leaf from that exhibited under less strenuous conditions of evaporation.

A consideration of evaporation as a controlling factor in plant growth would be logically incomplete without mention of the abnormal behavior of many pronounced xerophytes when subjected to conditions of low evaporation rate. Among gardeners the succulents are generally regarded as very difficult to grow in the more humid regions, and especially in glass houses. Such plants often fail to develop normally and often meet their death through the action of fungus diseases of the damping-off form. It is possible for the gardener to provide conditions of soil moisture very nearly approximating those of arid regions, but in most conservatories it is practically impossible to attain anything like the atmospheric conditions to which desert succulents are normally subjected for a great part of the year. I have had occasion to study the fine collection of cacti grown under glass at the Missouri Botanical Garden, at which place I enjoyed the privileges of working in the conservatories during two months of last winter, and it appeared that while many species were thriving well under the artificial conditions, yet a number of forms were not at all healthy. It was further determined that these plants were not transpiring in the normal manner. I have also often observed at the Desert Laboratory that pot-grown cactus seedlings are very prone to die of a damping-off disease or root disease, especially when subjected to a low evaporation rate. It would thus seem that certain of these plants which are adapted to great evaporating power of the air, do not thrive when exposed to low evaporation. Perhaps the transpiration stream is necessary in such cases. It may be that the epidermal covering does not develop normally in the absence of rapid transpiration, and that this explains the frequent destruction of these succulents by fungi when grown in conservatories.

It is possible that evaporation plays an important part in causing the marked differences in the vegetation of neighboring sunny and shady areas. In an experiment carried on by the Desert Laboratory in cooperation with a large number of workers distributed over the United States, a number of tests of the

evaporation conditions in sunny and shady situations have been made during the growing season just past. These tests uniformly show a much higher evaporation rate in the sun than in the shade. An instance of this difference may be taken from the readings of two instruments installed at the Missouri Botanical Garden, at St. Louis. For these data I am indebted to the Director of that Garden, for an active and appreciative interest, and to Mr. Henri Hus, for the care of the instruments and the preparation of the data. In this test one of the evaporimeters was exposed 15 cm. above the soil surface in a denuded area about three yards square lying within the Experiment Garden. From this area vegetation was excluded during the season. The second instrument stood at the same height above the ground in a coppice in the Arboretum, where the shade was rather dense. For the period from May 19th to June 17th, the average weekly rate of evaporation was 142 cc. in the sun, and 58 cc. in the shade. From July 22d to August 26th, the average rates were 187 cc. and 61 cc. Records for the period intervening between these two were lost on account of an injury to one of the instruments.

Attention was called to the importance of evaporation in the general distribution of forest centers by Transeau, in a paper in *The American Naturalist,* Vol. 39, pp. 875-889, 1905. The same material, in a condensed form, appeared in the Seventh Annual Report of the Michigan Academy of Science, pp. 73-75. These papers call attention to the fact that the distribution of forest centers in eastern America cannot be accounted for on the ground of heat and precipitation. The author presents a map showing the ratio of rainfall to evaporation, expressed in percentages, deriving his evaporation data from the paper of T. Russell in the *Monthly Weather Review* for September, 1888, and shows conclusively that this map exhibits "climatic centers which correspond in general with the centers of plant distribution. Further, the distribution of grassland, prairie, open forest, and dense forest regions is clearly indicated. . . . This is explained by the fact that such ratios [of rainfall to evaporation] involve four climatic factors which are of the greatest import-

ance to plant life, viz., temperature, relative humidity, wind velocity, and rainfall."

Dr. Transeau and myself devised a plan for obtaining evaporation data more definite than that at hand by asking the assistance of numerous workers in the country to install and operate the instruments for a widely distributed series of observations. The outcome of this plan was the cooperative experiment mentioned in a previous paragraph. Data are at hand for some thirty stations, but have not yet been sufficiently worked over to warrant their being made public here; they will be published at a later date. I am able to give at this time, however, a good example of the differences in evaporation of stations at different altitudes in the vicinity of Tucson, Arizona. Instruments were installed on May 12th, 14th and 16th, in the Santa Catalina Mountains at altitudes of approximately 6,000, 7,500, and 8,000 feet. These instruments were read on May 31st and June 1st and gave the following average weekly rates: 6,000 ft., 238 cc.; 7,500 ft., 147 cc.; 8,000 ft., 133 cc.

These instruments were placed at a height of 15 cm. above the ground and were all in the open, but were surrounded at some distance by the vegetation of the locality, the lower one by scrub oaks, the middle one by open pine woods, and the upper one by a denser growth of pine, Douglas spruce, etc. Later in the season these instruments were injured by the action of impure water, so that I am unable to give an average for a longer period. These figures would suggest that the evaporating power of the air plays a great part in the distribution of vegetation at the different altitudes of a mountain range of this sort, a conclusion which would be expected from the work of Transeau above mentioned.

The President—I am, myself, very glad indeed that this subject of evaporation is receiving this attention, which is indicated by Dr. Livingston's paper. I think we have not given a tenth of the attention we ought to give to this subject of evaporation from plants. I have not the figures at my fingers' ends about the amount of water evaporated by a tree in a day. We have statements of the amount of water evaporated by a field of grain, maze, in full vigorous growth. The amount of water evaporated by an acre of any plant, say of clover, or anything of that kind, is given to us, and it is staggering in the figures, and it certainly is a very im-

portant factor in this whole subject of plant development and plant life. The nursery trade generally will tell you the fall is the time to plant anything deciduous. You go through the nurseries and they are going all around—pulling the leaves off every tree. What is that for? It is to make the purchaser think that tree has shed its leaves and has got its season's growth, and secondly, they know if that tree is put up with those leaves on, the evaporation is going to exhaust the tree immediately and it will lose its life and it is going to die. The evergreen is a tree that carries its leaves the year around. It is not like a tree that sheds its leaves. There is a point to it, to which sufficient attention is not given, and I think it is extremely important because, as Dr. Livingston and Dr. MacDougal stated, there is a point beyond which, if the moisture of the plant is exhausted below a certain point, you have reached the death-point beyond which life cannot endure. By this investigation that has been indicated in this paper, I think we are going to get a great deal of light on these important matters.

Dr. Hansen—It has been our experience in our nursery work in the northwest (that means the northern part of the Mississippi valley) that the fall planting of all trees means death to a tree. They are usually dry enough to burn by spring. A tree has no chance whatever to establish communication with moisture in the soil, especially evergreens. It is sometimes said that our dry winter winds will take the moisture from a fence post.

The President—There is no doubt about it.

Dr. Hansen—Another point I have noticed in the spring is that the fruit trees in the nursery have the young shoots shriveled very much, and in the early spring it is unsafe to dig too much because the shoots are so shriveled that they lack moisture. They have had more moisture evaporated in the winter than there was in the twig, but by leaving them for a few days in the spring after a rain, they plump up and the shoots absorb enough moisture from the air, it is presumed, so that they have assumed a plump appearance. I think this question of evaporation is an important factor.

Mr. Southwick—I have in my notes here, a lecture given in this hall, in which it was stated that an elm tree evaporated 100 tons of moisture a day, and that an acre of grain—it does not say what was on it—evaporated one hundred tons of water in a day. Is that so?

The President—That is a remarkable statement. That is the trouble. A great many of us get hold of a truth and try to kill it in every way we can by exaggeration.

Mr. Siebrecht—I do not believe in late fall planting of evergreens. If you can transplant them early enough so that they will get at least three weeks or six weeks of warm weather with moisture with it, and you get nearly all the roots—I think under such favorable conditions, you can make our evergreens live. They will make small fibres, and if you will only take them along, they are all ready to spring. That is to

say they keep dormant until late in the season, and all at once the hot sun comes in the latter part (or the early part, I have seen it on the 20th of April). Then the trees begin to bud, I am speaking of evergreens now, and put out new roots. Now, as to shade trees, it is very true, that the people place an order in the spring or in the summer for trees and shrubs, and cannot wait. A gentleman called me up yesterday and said, "Why haven't you planted those shrubs? My trees are all in." Well, we had to take a lot of trees, lindens, and Norway maples, and we stripped off the leaves, in fact, we cut them off. I do not believe in stripping because you pull them out of the socket, as it were, so we cut off half the leaves and transplanted that tree with a nice bunch of roots and fibres, and after such a good rain as we have had lately, they stand up and are nice-looking. If they should make a growth and the growth should not get matured, then of course it would suffer during the winter; but the nurseryman has got so short a time in the spring that you cannot blame him.

Mr. Macoun—In Canada, the subject of evaporation is a very important one, especially in our poorer districts. We find, for instance, in the Behring Province, that the evaporation is too great for most kinds of trees, and most varieties of tree fruit, and what we wish to find out is just how much moisture each variety of fruit must have during the winter to enable it to stand the winter, because it is quite evident that a tree must have a certain average amount of moisture during the winter. On the contrary at Ottawa, trees which are hardy there, different varieties, only some of which are hardier elsewhere, we find the tenderer varieties of those kinds have the largest amount of moisture. The analyses of the chemists say that the tenderest varieties of apples, for instance, which were killed at other places farther north, had the highest amount of moisture at Ottawa, hence a different treatment would have to be given those trees farther north than trees which had not so much moisture. For instance, Dr. Hansen said in North Dakota they recommended cultivating very late in the autumn in order to get the trees as well charged with moisture as possible. I believe that is true in certain limits for certain varieties, but in the country where I live, if we were to cultivate these tenderer varieties in the late autumn, those trees would have too much moisture in them, and not be properly matured, and hence would be killed back at the terminal growth. I think that the point that should be worked out by those who are making a study of this question, is how much moisture certain varieties of trees should have on the average, to withstand the cold climate there. Now, we all have to grow a variety of apples, so as to cover the season, but it seems to me that we must give these varieties different treatment if we are to have equal success, and that is not a subject which has been given enough attention—the different methods for different varieties. We have been working on the subject at Ottawa, of growing these trees according to the degree of hardiness in proportion to results, and we find that the trees which mature

earliest are the hardiest, but those that are the least hardy have the most moisture.

The President—I would like to ask whether the subject does not bear directly upon what we call winter freezing. That is, the freezing and cracking of the trunk of a tree, which occasionally takes place, and which to me has always been inexplicable. Isn't that a part of this problem exactly?

Mr. Macoun—Yes, the freezing does this. Thus, in the Province of Nova Scotia, and I believe in a few places in New York, they have been troubled with what they call crown rot of a tree. That is the breaking away of the bark right at the base of the tree. In investigating this, I have come to the conclusion that that is almost entirely due to the late growth of the tree, and is due to the sudden breaking away of the bark of the tree, owing to the fact that right at the base of the tree you will have the most amount of moisture in the trunk. The first fall of snow prevents the drying out of moisture from that part of the trunk. That snow may go away and then there may come a drop of temperature to 30 or 40 degrees below, and the cracking takes place. In those parts where they adopt the highest method of cultivation, they are troubled the most. The late growth is the trouble.

Dr. Hansen—That must be the same trouble which we call "bark bursting." That occurs in nursery stock out in the northwest. The trunk becomes perfectly saturated with moisture, and it seems to fracture the bark at the surface. I did not mean in my former remarks to advocate late cultivation. I meant if we had a late fall we should give the trees a thorough watering before winter sets in.

Mr. Macoun—I notice that Professor Watkins advocates cultivating right up to the time winter sets in for hoed crops. I wrote to him but did not get any reply from him.

Dr. Hansen—One of our orchardists in Minnesota recommends the cultivation of orchards right up to winter when the snow comes, but what he means is not what we mean by cultivation. He simply means the use of a disk harrow to scratch the surface and keep in the moisture.

Mr. Von Herf—I have also had some personal experience in regard to plums, apples and peaches, and also grape-vines in North Carolina. The trouble does not occur every year, but it occurs in some years, sometimes being very disastrous. Sometimes hundreds of thousands of trees will be killed. I know of only one year, however, when grape-vines were killed in that way. Some were killed in the ground and others sprouted out, but were killed dead. We found the stopping of cultivation at certain times in the fall is a good precaution against this trouble, because we do not think it necessary of late to cultivate after the tree has made its growth. We think it is for no purpose. We stop cultivating about August. In regard to transplanting the trees, I have also some experience and I can corroborate what Mr. Siebrecht says, that the transplanting of evergreens is most successful just at the period when they

are sprouting out. We transplanted with certain success along these lines, magnolia and other things. I can also cite an example in Washington. They have a nursery for street planting only and I was told some years ago by the gentleman in charge that they could never successfully transplant an American poplar. This is not an evergreen, but it seems to make the same demand as evergreens. He says they have no trouble now, if they transplant at the time it is sprouting out.

Mr. Siebrecht—Talking about excessive moisture, there is an example. The tree wants to be handled like an evergreen. If you take it at just the time the bud is swelling, you can transplant any size of tree, but you take it too early or too late, and you lose every time. Speaking about the excessive moisture and cracking of the bark in trees, I want to say just a word. I have found in my nursery—I have got all kinds and conditions of soil, upland and sandy soil and flat land and heavy soil—the heavy soil I must not cultivate late in the fall. There is plenty of moisture there. There is too much. I planted trees last winter, and many of them cracked up and down. That was late in the season, and too much evaporation took place, more than the body could contain, and the bark split. With the Norway maples, it was the same way: of those upon the high ground and the hillside, not one suffered, although they are not so luxuriant and the growth is slower, but the foliage was heavy. There is no cracking, either with the Norway maples, linden trees and plane trees. I find we have to study the conditions of the atmosphere and the ground and the locations for the different kinds of trees, and where it is a dry climate, with the climatic conditions on the dry side, late cultivation might be very well to give the tree all the condensation and the moisture you can. On the other hand, where it is very moist and wet, leaving it alone, letting the tree mature and letting it have its own way is a good thing; and then it can stand the winter in a dormant or semi-dormant condition.

Mr. Munson—I think a portion of this question is a matter of evaporation and condensation. I have had an experience of that kind. For example, the transplanting of the magnolia, one of the most difficult trees to plant if the foliage is left upon it, but by clipping the foliage off, they can be transplanted almost as easily as beech trees. It is true that evaporation enters into that problem, but why does it not transplant as readily as arbor vitæ at that season?

Mr. Siebrecht—You have got too much moisture to take care of. My observation leads me to this conclusion; that the magnolia and most of the evergreens are very slow in starting to feed the roots. It takes a larger amount of temperature to start the new roots to feeding, and you have got the tree loose from the soil and it cannot make feeding roots quickly enough, so in the meantime the evaporation from the foliage exhausts the tree. We take that evaporation away so that it can stand long enough for the roots to form and then it can feed and it is all right. If you want to transplant a tree in full foliage so as to make a showing

at once, you must provide shade for it so as to prevent evaporation. Now, it works differently whether the tree be dormant or active. I have observed frequently that while in the spring we cultivate and work up a plot of ground, say for a peach tree, we push them into bud and bloom more rapidly than if we had not disturbed that soil. The evergreens cultivated in the same way will push a little more rapidly and become filled with sap sooner and the sharp freezing following, we lose the first crop of buds and shoots. They are more sensitive to frost. That is the young growths when they are full of sap, are more sensitive to frost than when they are dormant. When they are dormant, it seems that they are more hardy.

The President—Reference has been made to transplanting Magnolia grandiflora. I would say I have been laboring for fifteen or twenty years to acclimate it to this latitude, because it is, as you all know, the grandest magnolia in the world. So far as I know, the most successful specimens in this part of the country are at Riverton, New Jersey, a few miles north of Philadelphia. If there are any north of that, I would like to know it. I think this region is the center of the world, and that this place should not be satisfied until we get Magnolia grandiflora here, so I tried with a great number of specimens, but never could get them through a longer period than five years, and never got them to bloom. I thought by growing a number of specimens and protecting them well the first winter, a little bit the next winter and a little bit less the next winter after that, I should, by the fifth winter succeed, but unfortunately, I was like the good German who said he got his horse down successfully to a point where he could live on one straw a day, and then the horse died, so when I got this along to the fifth year, it died. So this theory of adapting the tree to its environment by slow process has not been successful with Magnolia grandiflora. The average temperature is just as low at Riverton, N. J., as it is at my place, although I am a good way north and there are other conditions affecting the surroundings, so it is not temperature alone. I don't know what it is, nor do I know what is the matter with the magnolia. The chief factor, however, I do not think is temperature.

Mr. Von Herf—I found a great difference between individuals. We have to consider that practically all of the magnolias we see are raised from seed, and if you pay attention, you will find there is as great a difference as between seedling apples, or any kind you grow from seed. Some grow tall and some not so tall. Some have broad leaves and others have leaves which are narrow as a laurel, also they differ in their capacity to bloom, and they differ in the same way as to hardiness. I found a large number of such seedling magnolias in a section where they are frostbitten, and some so tender that they freeze to the ground. These are individual specimens. Now, the most northern I have seen were in Philadelphia, and right in the city. I was recently in the city and I saw

the place where it had been cut down, and it must have been very large, because it was a large trunk.

The President—It was on the corner of Broad and Chestnut Streets, and there is a seventeen-story building there now, and the two were not compatible. That is why they did not get on together.

Mr. Von Herf—The way is to select seed from the most northern trees and push them on in that way. I think it can be so arranged in New York as to get them in sheltered locations. I observed some Magnolia grandiflora growing in Washington City. One would think that as good a locality in which they would grow well as any place, but they do not grow there as well as they do a little bit farther south. I have in mind some trees opposite the White House. I saw them twenty years ago, and they appear to me now scarcely any larger than they did then. Now, in a proper location, they would be much larger than they were then. We can hardly expect to raise magnolias as fine as they are in their home, but I think they could be grown by proper selection in the manner I indicated.

Mr. Siebrecht—I suppose it was nearly twenty years ago when a lady, a customer of mine, offered me a thousand dollars if I would make a magnolia grow in 54th Street in front of old St. Luke's Hospital, and I declined the offer. Since then I have been practicing, like our president here, and at last I have got some that high (indicating about 4 to 5 feet), and those are from seed brought from Mount Vernon. Mr. Stuart, the superintendent of the grounds at Mount Vernon, said, "If you do not make these grow north, then it cannot be done. These are the most hardy we have." I have got them up to now, and I believe what my friend says. I have tried in the same way with English holly and have been fairly successful with the variegated and the laurel leaved, and also the common one in England. I have also succeeded in acclimating aucuba. It is doing very well. I have also acclimated the laurel and the big leaved laurel. I have got them but they are not very big trees and they are in sheltered locations, of course. I have succeeded in wintering over the crepe myrtle, and although they killed down to the ground very often, they are in flower now. Therefore I think we can, by trying, acclimate those things.

The President—I think that if I could get seedlings from trees that were native in North Carolina four thousand feet above the level of the sea, where the thermometer sometimes gets 25 degrees below zero, Fahrenheit, we ought to have them here, and I have brought them from four thousand feet above sea level, North Carolina, with the expectation of doing so. I have also had them from New Jersey and they did so well that they not only thrived, but perfected their seed, so I get seedlings from them and it would seem as though 4,000 elevation in North Carolina ought to bring it to New York successfully.

Mr. Von Herf—What place is that?

The President—Near Cranberry, southwest of Asheville. I hope in time we will have some publications from Tucson on this matter of evaporation which will throw some considerable light on this subject. I cannot understand how it is that nature overloads the trees with moisture in the winter time. It reminds me, while this matter is being discussed, of a man up in a little town who went by the name of Uncle Jabez, his name being Jabez Jones. He was like these trees which the winter affects—occasionally he would get overloaded with moisture, alcoholic moisture—and one day he came out and he evidently had too heavy a load on, and someone says, "Uncle Jabez, you are overloaded to-day," and with a twinkle in his eye, he said, "Yes, I would better have made two trips!" (Laughter). Now, Mr. Munson, let us hear about "Resistance to Cold, Heat, Wet, Drought, Soils, etc., in Grapes."

The following paper was read by T. V. Munson:

Resistance to Cold, Heat, Wet, Drought, Soils, Insects, Fungi, in Grapes.

By T. V. Munson, *Denison, Texas.*

As a general, if not a universal, law of adaptation of plants to environment, we find that the natives in the environment are better adapted than the exotics.

The vine is no exception to this law. Let us test the assertion by comparison.

What species and varieties of grapes resist winter's cold best? Certainly the vines, and vineyard varieties derived therefrom, native in cold regions, known as *Vitis vulpina (riparia)*, northern section of *V. labrusca, V. cordifolia,* northern section, *V. bicolor, V. cinerea,* northern section, *V. rubra.*

Of these, *V. vulpina* of Wisconsin, Minnesota and Dakota readily endures without protection, —40° to —50° F. But *vulpina* of Virginia and northern Texas can endure only —15° to —20°. The Labruscas of Massachusetts can withstand —20° to —25°, while those of South Carolina perish in —10° to —15°. The *cordifolia* of central Illinois and Ohio (about the northern limit of this species) endures —15° to —25°, while the Florida *cordifolia* is sometimes killed to the ground in northern Texas with zero or a few degrees colder. *V. bicolor* of southern Wisconsin endures —30° readily, while Norton Virginia, of the nearly allied species of *aestivalis,* finds its northern limits about Louisville and Cincinnati. So we might continue with all the species and their varieties.

The limiting lines of hardiness to withstand cold do not follow the parallels of latitude, but the isothermal lines. Hence we have wild grapes in northwest Texas that readily endure the winters of Massachusetts, and the *Vitis Californica,* found along Rogue River in southern Oregon,—its extreme northern range—winter-kills to the ground, when grown at Denison, Texas.

The Post-Oak grape of northern Texas endures the winters perfectly in middle Ohio, where temperature sinks to —25° sometimes. But when we come to reverse the test, the Massachusetts and Ohio grapes cannot endure the Texas summers anything nearly so well as do the native Post-Oak and Mustang grape. The Concord, that remains vigorous for fifty years in Massachusetts, its native State, survives only eight to fifteen years in Texas with equally good treatment. This brings into consideration another element of hardiness, the power to withstand great or only small range of climatic change. In this, the general law still holds good. Those species with their varieties native in a region subject to great and sudden ranges of temperature, can endure well, while those brought into such regions from where the range is small, will suffer, as is the case with grapes of Florida or New England brought into northwest Texas. Both suffer, while the vines of northwest Texas thrive well, both in Florida and New England, so far as resistance to heat and cold are concerned. In other words, northwest Texas plants have a much wider range of climatic endurance than have either those of Florida or Massachusetts.

No other section of the United States has so great a range of climatic conditions as northern Texas and Oklahoma.

This will suffice as to cold, heat, wet and drought; but we must not fail to observe, that some individuals of a species have greater resistance power than others, all developed in the same climate, soil, etc. This fact is seized by the plant breeder with great avidity, to increase the hardiness of his varieties of same blood and nativity by selection, depending on the law of inheritance to sustain his selections.

As to soil, the law holds good so far as resisting an excessive or injurious chemical element. For example: some varieties of grapes, the Labrusca, Lincecumii and Rotundifolia varieties, especially, chlorose very badly (turn a pale, sickly yellow in foliage), if set in soils having above 40% of carbonate of lime, while the Vinifera, Cordifolia, Cinerea, Berlandieri, Champini, Candicans, Rupestris and Monticola thrive in such soils. We find those that chlorose badly are natives of very sandy soils, along the banks of streams and lakes—the Vulpina, or on sand-

hills, as is the Post-Oak grape of Texas, while those that grow best in very limey soils, belong to species native in such soils.

We find, however, that in some cases varieties of species, native in very sterile soils, take on far more vigorous growth when put into soils richer in humus, and the chief elements of plant-food; and this causes excess of wood and leaf-growth, to the detriment of fruit bearing, when carried to the extreme.

Nearly all species of grapes are native in warm, loamy, well-drained soils, and such cannot long endure with roots permanently in water, or in cold, livery, compact clays; but a few species are known that cannot long survive in soils not sub-irrigated, or having growing moisture at all times. Such are Vulpina, Rupestris, Cordifolia, Cinerea, Rotundifolia and Simpsoni. The last-named is often found with the roots perpetually submerged in the borders of swamps, and making immense growth. In such situations, the Vinifera, native on the limey hills of southwest Aisa, and Post-Oak grapes of the Texas sand-hills, would survive only a short time.

In land that is seapy during rainy weather and some time after, but in the dry, hot summers, dries out and becomes hard, no grape thrives. Cordifolia, above all other species, perhaps, can endure such situations longest.

Concerning resistance to mildews, rots, etc., it is true that all species native in high arid regions are very quickly and destructively attacked by the cryptogamic parasites, when moved into humid situations, where such organisms exist. For resistance to these parasites, natives of the parasitic regions must be sought. Perfectly resisting varieties in such regions, when hybridized with nonresisting varieties, produce only partially, or weakly resisting varieties.

Take a vine from a parasitic region, loaded with the parasites of mildew and rot, and plant it in an arid region and it becomes free of these fungi, simply because the parasites must have much moisture in the air to propagate.

This law does not hold good as to root parasites, or bacterial blights that live within the cells. For example, the Anahein grape disease, of California, thrives in the moist regions of northern California as well as in the dry region of southern Cali-

fornia, where it originated, just as pear-blight, when once introduced into California and Colorado, is as contagious and destructive as in Georgia or Texas.

The insects that infest grapes know no specific bounds. The rose chafer, the fidia, the leaf-folder, the leaf-hopper, are just as bad in one region as another in which they can endure the winters, and on one species as another with few exceptions. Certain varieties resist, or are distasteful to these insects, and thereby escape, while others are greatly liked and damaged by them.

The leaf-folder, however, never hurts a vine that has leaves that are glabrous, that is, entirely without pubescence, or down of any kind on the upper side of the leaves. The egg is laid on the upper side and the larva finding no pubescence to tie its webs to, and thus unable to draw the leaf together over it, soon perishes in the sun, or is eaten by birds; hence only grapes with leaves more or less downy on upper side of the leaves are damaged by the leaf-folder.

There are some varieties of grapes much less bothered by the leaf-hopper, than others. These generally are the varieties with very firm, dense tissue, such as the Post-Oak grapes of Texas. The fidia and rose chafers make little choice of kinds, and are voracious feeders on the foliage. The Rotundifolia is freest from attack of fungi and insects, in fact almost entirely exempt.

The Phylloxera comes well under the general rule. It can do little damage to those species of grapes native in the same regions where the Phylloxera is native, yet there is much difference in resistance there. For example, *V. rotundifolia* is entirely immune; *rupestris, vulpina, cinerea, Berlandieri, Champini, candicans, Doaniana, aestivalis* and *Lincecumii* are so high in resistance as to be practically uninjured, though they may be attacked; while *Labrusca* is low in resistance and is much weakened in clay soils, if infested, and *vinifera* is entirely non-resistant. It is a native of regions never infested by Phylloxera, until introduced among cultivated vines.

To have given full lists of resistant and non-resistant varieties under each heading of this paper would have been entirely

beyond its province, as from what is said, any one acquainted with grape species and their native habitats can readily select the resistant kinds in each case.

The President—The subject is now before the Conference for any discussion you may have upon it.

Mr. Von Herf—With regard to heat and cold, I experimented for nearly ten years with about 350 varieties, and they stood the winter very well, with the exception of one variety, which was very tender, which was the plant we got from the White House in Washington and which seemed to be quite tropical in its nature. Now, as to the resistance of vinifera, we had no trouble, because it was sandy soil, and they were mostly grassed and some got their growth. I believe if we could suppress the black rot on vinifera, they would grow just as well. They require more careful treatment and better fertilizing, and more care, but they will grow. There is no trouble about that. It is only the black rot which makes it impossible to raise fruit. We got vines from nurseries for a year or two, but the trouble got worse every year until we found it practically impossible to continue although there is a little difference in the different varieties, but we cannot even raise Niagara after the vines have reached some age. That is, after they are some three or five years old. The Niagara, I am told, has the vinifera in it. I think we produced vinifera from seeds, the vinifera vine from seeds coming up from Niagara grape seeds, so I think it is shown it has vinifera in it.

The President—Dr. Hansen has a paper which he will read entitled, "Is Acclimatization an Impossibility?"

The following paper was read by N. E. Hansen:

Is Acclimatization an Impossibility?

By Professor N. E. Hansen,

South Dakota State College of Agriculture and Mechanic Arts, Brookings, South Dakota.

The title of this paper would seem to indicate views decidedly iconoclastic and heterodox for a Conference on Plant Acclimatization. The casual reader might inquire why we should hold a conference on how to accomplish an impossibility. This line of thought has accumulated slowly in the course of extensive horticultural experiments extending over a series of years, and I will expect some objections from people who have never faced the problem of originating fruits capable of enduring —40° F. with the ground bare of snow. I do not believe in giving winter protection to any plant, because that is *horticulture on crutches* and hence something to be avoided if possible.

The argument may be divided into the following sections:

A. Acclimatization of perennial plants; Botanical names are insufficient for horticulturists; DeCandolle's law. What is possible for nature is impossible for man.

B. Acclimatization of annual plants; Easily done by man, but is really a process of sifting out of unfit elementary species; or minor changes, such as shortening the period of growth.

Horticulturists who have had to deal with plants in the prairie northwest have learned by costly experience that the source of seed of any variety of tree, or any other perennial plant, determines in a large measure its hardiness in a given locality. Hence my contention is that botanical names do not tell the whole story. Box elders from Southern States winterkill at the North. The people of Manitoba have learned long ago that they should not plant box elder seed native of the South,

although they appear to be similar in all observable botanical characters. Russian foresters called our box elder tender, the seed having been gathered near St. Louis, Missouri; but when they secured seed from pure native trees in Manitoba they found the trees to be perfectly hardy at the north. They named this variety "Boreale," indicating its northern origin; as yet we have not waked up enough here to make this distinction as far as I am aware, and the average planter is blissfully ignorant as to whether the box elder he plants dates back to Arkansas, Manitoba, Missouri or North Dakota.

It has been found by many northern nurserymen that the red cedar of the South is tender and short-lived at the North, while the local northern form of the species is hardy.

Robert Douglas learned many years ago in northern Illinois that the black walnut of the South was tender at the North, whereas the local northern form was hardy. He also determined that there was a decided difference in hardiness on exposed prairies of the conifers from the Rocky Mountains; the forms of various species from the Pacific slope side being much inferior in hardiness to those from the eastern slope.

In the course of three trips to Russia I learned that Russian foresters had found the Scotch pine from western Europe, and from France and Germany, inferior in hardiness to the Scotch pine of northern Russia and extending into Siberia. Similar differences have been noticed between the west European and the east European and west Siberian form of the Norway spruce.

Many more instances might be given of the fact that a species extending over a wide geographical range, varies widely in its capacity to resist cold. It shows then that nature must have done a work in acclimating plants so that it will endure a greater degree of cold.

DeCandolle, in his "Origin of Cultivated Plants," states: "The northern limits of wild species . . . have not changed within historic times, although the seeds are carried frequently and continually to north of each limit. Periods of more than four or five thousand years, or changements of form and duration, are needed, apparently, to produce a modification in a plant

which will allow it to support a greater degree of cold." This proposition means that plants cannot be bred so that they will be more resistant to cold by ordinary selection, as it would take too long. For hundreds of years on this continent, for example, we attempted to originate hardy grapes, apples, raspberries, plums and sweet cherries from the species originally brought over from the mild climate of southern and western Europe, but no success was obtained. Thousands of orchards, vineyards, and small fruit plantations, wrecked by our test winters in the prairie northwest, show that man's efforts at acclimatization were in vain. He could not secure a hardy plant from a tender one. In other words, we were starting on a ten-thousand-year job. Nature demands a century of centuries for the completion of some experiments. She can do such work, we cannot.

When it comes to the acclimatization of annual plants the problem is in the main of a different nature. A large late southern variety of Indian corn when moved too far north will of course be cut off by early frosts. If moved not too far north there is sure to be some extra early strain within the variety which the observing farmer will select for his next year's seed, and in a few years he will have an extra early variety adapted to his locality. By always selecting for earliness the Indians were able to carry corn, a semi-tropical plant, from South America north to Manitoba. This selection was accomplished by the Indians before Columbus discovered America. The Indians cured their seed corn by hanging it up in the smoke of their teepees to dry: and the best modern methods of curing seed corn by artificial heat are in a sense no improvement on this aboriginal method. In parts of south central America corn attains a height of twenty feet with kernels several times larger than our northern seed kernels, and it requires seven months to mature. At the northern limit in Manitoba it is perhaps five feet in height and requires three months to ripen, but in all this time we have simply shortened the season. Corn has not changed its nature in its requirement of extreme heat for ripening seed. As yet we have no corn that will endure frost to any extent, nor has northern Europe been able to originate a variety

from a variety from our Indian corn that will ripen. A new light has been thrown on this matter by the mutation theory of De Vries. We now know that a systematic species is made up of a great many elementary species which are perfectly distinct from each other and absolutely fixed in type. While on my third trip to Russia in the summer of 1906, I visited the experiment station at Svalöf, Sweden, where Dr. Nilsson has been conducting with such remarkable success a series of experiments in improving cereals by isolating each variety as an elementary species. The Swedish Select oats, for example, which is now so popular in the prairie northwest, was originated at this station. Dr. Nilsson considers that an ordinary variety of barley, for example, is made up in reality of many varieties differing slightly but each perfectly distinct from the others and constant from seed. These sub-varieties are really elementary species, i. e., they are mutations. When grown in certain sections for many years these hold their own in varying proportions from year to year; but when raised at the South, the extra early mutations will be crowded out to a considerable extent by the later mutations, which are usually more productive. If now this variety is transferred say four hundred miles further north, a readjustment of these varying proportions takes place very rapidly. The late mutations are crowded out because the seed does not mature, while the extra early ones, which were in a decided minority before, quickly gain the ascendency and very soon the variety is made up entirely of these extremely early sub-varieties. So that the acclimatization of annual plants is in reality not a changing in the plants themselves, but only the sifting out of the unfit elementary species. In fact, Dr. Nilsson believes that extra early mutations can be isolated at the South, as well as they can be at the extreme North, provided sufficient care is taken in the selection. The farther south a variety is raised, the greater the care needed to select only the very earliest mutations, if a variety for the far north is desired.

It is worthy of note that the many failures in farming in the cold, semi-arid regions of the prairie northwest are due to the fact that the plants cultivated were from the milder climate of western Europe. In other words, it is unwise to farm in a

dry, cold climate with wet, warm climate plants. This is a very fundamental proposition. The farmers of America have spent hundreds of millions in the vain effort to acclimate certain plants. Let it now be placed on record that my study of horticultural problems in the prairie northwest has taught me to have no faith in the possibility of acclimating plants to a greater degree of cold than that to which they are accustomed in their original habitat. I believe that those who are attempting to acclimate the common alfalfa, brought over from northern Africa by the Spaniards to South America and thence to California, to the conditions of our prairie northwest, are starting on a ten-thousand-year job and hence impossible. This led me as the first agricultural explorer from the United States Department of Agriculture to attempt in 1897-1898 an overland trip of two thousand miles through northern Turkestan, western China and southern Siberia in the endeavor to find a hardy form of the common alfalfa. Blizzards interfered and the trip was a severe test of endurance. As a result of the trip Turkestan alfalfa was brought to America for the first time for the spring of 1898. This initial exportation of eighteen thousand pounds I understand from good authority, has now risen to nearly five million pounds exported annually, the larger part of which goes to South America and about a hundred and fifty tons to the United States, and the amount is fast increasing. Turkestan alfalfa has proven to be more resistant to cold and drought than the ordinary alfalfa. But I was not satisfied that I had secured the most northern form of the alfalfa, so in a six-months' trip in the summer and fall of last year, I made another attempt and found that there were other species, yellow flowered, extending north of the common alfalfa limits one to two thousand miles across Asiatic Siberia. I only hope it will help to solve the alfalfa question on the American continent and that it will carry the alfalfa belt north as far as we care to farm.

SUMMARY.

My belief concisely stated is that to breed plants so that they will endure a greater degree of cold is a work demanding such a great period of time that it is for nature rather than

man to undertake. The only way in which it might possibly occur is by some great mutation but so far, out of many instances, as in the case of the apple, raspberry, grape, alfalfa and clover, no noteworthy progress has been made in making plants hardier by selection from tender stock. The occasional test winters, such as that of February, 1899, in which millions of dollars' worth of alfalfa were destroyed, indicate that it is not feasible to get hardy plants by selection from tender plants. In each and every case it is starting on a work that may take many thousands of years for completion, and the test winters may compel us to begin all over again. Nature takes a century of centuries for some experiments. Let us leave such work to her.

But, you may ask, how may we get hardiness into tender plants? I look upon hardiness as something easily transmitted in crossing and I am working extensively along this line in my work of raising seedlings of native northwestern fruits by the hundreds of thousands. Hardiness may turn out to be a unit character obeying Mendel's law of heredity. In plants, such as plums and apples, which are propagated by some mode of division of the original plant such as budding or grafting, we will not need to go further than to secure the original hybrid that has the characters we desire. But this line of thought is not within the scope of this Conference, as we are discussing acclimatization and not hybridization.

The President—The paper is now open for discussion.

Mr. Siebrecht—I wish to say that as a boy and a young man, very often I grew Indian corn in the University in Germany, from English corn. There were thirty varieties that were adapted. That was in northern Germany, in Hanover.

The President—I think that this challenge from Dr. Hansen about this law means that no annual is admissible as an illustration. Dr. Hansen himself has spoken about the Indian corn. Indian corn does not bear at all upon it. It must be something that can bear through the winter, so when he spoke of this law, I said to myself, there are a thousand illustrations, but as I thought of it, I realized that no annual counts, because an annual does not bear through the winter, and no hybrid counts. And so when you eliminate the hybrids, it is the kind of law which you cannot meet, because you are always uncertain of your ground. While he spoke I thought of Indian corn, and I thought of a score of things, but they are not admissible. Now, let us see what we can

find. So far as our information goes, the cherry is a native of Persia, and here is an illustration which conflicts with the law. When I have eaten such magnificent cherries, as I have in Scandinavian countries and in England, the same latitude as our Labrador, and find a thing which we have no thought of hybdridizing, I think that the law is met there completely. So far as we know, the peach is a native of Persia, with the exception that we have found it is also apparently a native of Manchuria. Whether the Persian peach came from Manchuria, or the Manchurian came from Persia, in prehistoric times, we do not know, but I think we have a right to assume that the peach is a native of Persia, and the Manchurian peach in some way strayed from Persia. If that thing it true, there is no comflict of the law. So far as we know, the currant is a native of the southern countries of Europe. It gets its name from Greece, and the currant is a native of Corinth. This is one of the laws which eliminates evidence. It is like the criminal trial to-day, in that you cannot introduce any evidence;—this is eliminated and that is excluded, and you have got nothing and the fellow gets free; so it is with this law, when you come to think about it. I am going to stand by the cherry and the peach and the currant, unless you can prove their origin from somewhere except the historic origin.

Dr. Hansen—You will have to eliminate the currants, Mr. President. But the currant which we buy in the market, and is grown on the coast of Corinth, is not a currant at all, but a grape. But our currant is a native of that region. We call it a currant, and what we call a currant comes from Asia Minor.

Mr. Munson—I think the position of Dr. Hansen can be sustained, if he would only give us the definition of procedure. If he means that I should pick a half a bushel of wheat out of a pile grown upon a field of wheat, indiscriminately, and sow it, and from the pile grown on that field, take another half bushel and sow that, and so on, it might take four or five thousand years to get a hardy wheat, and I can hardly conceive you would get one very hardy then.

The President—But you have got to get your wheat and leave it out of doors during the winter. You have got to have that seed carried through the winter.

Dr. Hansen—Let us have the whole thing. Give us the whole thought, whatever you have.

Mr. Munson—Take indiscriminately a lot of apple seeds out of an orchard, and plant another seedling orchard, and then take the seed out of that indiscriminately, an equal quantity, and continue to plant in that same quantity right along. You may take the seedlings from the orchard a thousand generations ahead, into a hardier climate, and they would be no more inured than the first one you started from. Now, I am on the Doctor's side that far, but suppose that I take that half bushel of apple seeds and plant an orchard of it here or in Texas and out of the most vigorous of those trees, I take a like quantity of seeds and plant them in

an orchard in Kansas, I would not have as many trees in that orchard for a few years as I would in Texas, if they were adapted to Texas in the first place, I suppose. I take half a bushel of them and plant them in Iowa or Nebraska and continue to travel northward, I will have less and less trees, but I will have some apple seeds that will end after several years in having apple seeds that will survive in Iowa and Nebraska, and this, if we accept it for your application, will kill all our experimental work. We stand upon the foundation of the survival of the fittest in all directions. Take the iron pea of the south, a selection out of all peas, the common pea, the Whipple pea, etc. Out of all varieties of them, one was found that resisted, and that was selected, and now we have them growing in fields full of nematode and not a pea touched. We are using it as fast as we can get the seed. It has not been known to be attacked by the nematode.

Mr. Siebrecht—Is this the same as the cow pea?

Mr. Munson—The same as the cow pea, known as the iron pea. Now, we know this, that there are certain varieties of vinifera grape in Texas where we go as low as 15 degrees below zero, some fifteen or twenty varieties that have stood that temperature, while there are others that will not go below zero. Now, I am sure if I should take that one that has endured the most cold and take pure seed—that is, its seed fertilized by itself—and plant, say in Arizona or Kansas, I might find a few that would stand the winter there; or find some hardier in that lot, and continue to plant a few generations farther and farther north, I would find a hardier crop, but I would not have a species. A transformation to another species is impossible. I am confident of that, but it is these little selections and the selecting as rapidly as the breeding of the plant will permit, that enables us to make any progress, and it is through that, that we do make all our progress with reference to this matter, I believe.

Dr. Hansen—I have taken the so-called hardy peaches of Iowa that have been raised for forty years by the seedling method, and have tried those in Dakota. The trouble is that I don't get a single individual to stand the winter. They all get killed. I don't get any to stand. I have to start over again. The western people tried our box elder and they winter-killed. They were about to give it up and they got the seed from Canada and it winter-killed. Then they got their seed from St. Louis. There is an example where they began too far away in the first place. What do you mean by a few generations? The generation of vinifera seedling would be how long? Five years?

Mr. Siebrecht—Four or five years in its natural course. You could make it bear in three years.

Dr. Hansen—Ten generations would be fifty years. The only trouble about that peach is, are we sure about Persia being the natural home?

The President—We do not know.

Dr. Hansen—They are a hardier form. I believe northern China. Is not that considered about the native home rather than Persia?

The President—We cannot tell. Of course, it is Persia or Manchuria.

Mr. Siebrecht—Something just came into my mind about English walnuts. I know an English walnut tree that is, I believe, fourteen or fifteen inches in diameter, the stem or trunk, and I believe a perfect tree, and which bears anywhere from four to seven bushels a year, and that stands right on the heights, not very far from Andrews' Monument, Tarrytown, part of the old place that formed part of the estate of Pocantico Hills, belonging to John D. Rockefeller. I guess it has been sheltered since it was a little tree put out there, by some spruce trees and pines planted to the northwest of it. No doubt they have sheltered it somewhat, but that is a perfect tree, and where trees have been brought from down south and from the other side that would not grow here, walnuts taken from that tree as grown here, have proved hardy. That sides in with what Professor Munson says, that you can make it hardier and hardier. That tree stands there and it is the finest in this country, I don't care where you go. There are some down in Freehold, New Jersey. I planted some there twenty or thirty years ago, and they are good, but not so good as this. There were some good ones around here, but a very cold winter such as we had a few years ago, killed them out; they start again however.

The President—We do not know where the original rose-bush grew, as it grew in the garden of Eden, but there are certain varieties, such as the China rose, and others—but the difficulty with the rose is that we have no pure rose to-day. They are all hybrids. There is the Bourbon rose. The Bourbon rose is the most distinct and probably the purest-blooded. It originated on the Island of Bourbon, and it is in conflict with this supposed law. That is a very good illustration. There is the pecan nut of America. The pecan nut is believed to be a native of Texas and has strayed up along the Mississippi valley until it is found in Missouri, and isn't Missouri the most northern limit of the pecan?

Dr. Evans—The Wabash Valley, Indiana.

The President—I have never seen any pecan north of Missouri. I was going to give that as an instance of it, but we do not know, of course, that it has come up there from Texas.

Mr. Munson—I have seen it growing in the valleys of Illinois.

The President—I have got pecans growing here. I was going to cite that as an instance, but if they have got them in Missouri, why then, we don't know. Then beside, so many of them are hybrids. Let me instance that, since Mr. Siebrecht spoke about the English walnut. The "English" walnut that we know is the walnut that we get from the Mediterranean. We do not get any from England. They are called Grenoble nuts. We get our best nuts from South Africa, and the northern shore of the Mediterranean. If it is hardy enough to live, it is not hardy enough to fruit. It does not fruit properly under ordinary conditions. Now, the black walnut belongs to the same family, one of the hardiest trees. A few years ago I conceived the idea, now, why not

raise hybrids from the black walnut and the English walnut? There is no difficulty about the crossing, and now with a sufficient number of specimens, we can get enough with the hardiness and vigor and fruitfulness of the black walnut with the quality and thin shell of the English walnut. So I went to work to have hybrids made, and now have several hundred growing. I picked the pollen from the English walnut and fertilized the blossom of the black walnut, and took the pollen from a black walnut and fertilized the blossom from the English walnut, and I have these trees growing now. If I can get out of three or four hundred specimens, one tree that has the hardiness and vigor and prolificness of the black walnut, with the quality of the English walnut, I will have lots of fun, but you see I will get nothing as an illustration, because the moment you get to the field of hybridization, you are outside of this matter. I do not think you can go by the law. It is like a great many other laws, the spirit may be good, but the letter of the law you cannot hold to. Now, right in this line, we have Dr. Evans of the Department of Agriculture who has a paper on "Experiments in Plant Acclimatization in Alaska," to present later and it may throw some light on this subject.

Here is a paper on "Developing Hardy Fruits for the North Mississippi Valley," by Mr. Samuel B. Green. Mr. Barron, our Secretary, will read that paper.

Gentlemen, I regret that I must leave you now, but I would ask Mr. Siebrecht to take the chair.

Mr. Siebrecht takes the chair.

The following paper, by Samuel B. Green, was then read by the Secretary:

Developing Hardy Fruits for the North Mississippi Valley.

BY PROF. SAMUEL B. GREEN,
University of Minnesota.

The varieties of fruits that were introduced into this country by the original settlers, while they often proved of value for a few years, have generally been superseded by better kinds that have originated in the section in which they are grown. That portion of the United States commonly known as the Central Northwest, including the States of Wisconsin, Minnesota, the Dakotas and northern Iowa and Illinois, have had more difficulty in getting varieties of fruits suitable to their conditions than perhaps any other portion of this country. This has been due to their cold winters and especially to an occasional extremely cold winter in which the ground is bare of snow. The climate of this section too is generally drier in summer and not as well adapted to the fruits of western Europe as the portions of the United States lying east or even those sections near the west coast.

One of the most important of these horticultural problems is presented in the apple, the old varieties of which are not successfully grown in this section. About forty years ago Peter Gideon undertook at Excelsior, Minnesota, to solve this problem by combining the Pyrus baccata and its hybrids with the Pyrus Malus, and thus getting hardiness and good quality. In the course of this work he discovered the Wealthy apple, which is to-day a leading variety over a large extent of country; but the dozen or more other varieties (which are undoubted crosses between the P. baccata and P. Malus) that he sent out as being especially adapted for the cold Northwest have proven unsatisfactory in many ways, largely on account of their susceptibility to fire blight. The work which Gideon undertook to do was

done in such a thorough way that there is no necessity of repeating it. The only important variety of apple which he did originate has been of so much value to the State of Minnesota, and elsewhere, that it has repaid a hundred fold all the money the State of Minnesota ever put into the experiment.

Other devoted horticulturists have put their time and means into similar lines of work, and often with very little in the way of remuneration except the consciousness of having accomplished something for the general betterment of mankind which, for most of these men, was reward enough. Among those who have especially benefited the pomology of this section by originating apples is Chas. G. Patten, of Charles City, Iowa, whose most important contribution has been Patten's Greening, but who has also introduced most excellent varieties in his University and Iowa Beauty.

H. M. Lyman, of Excelsior, Minnesota, began about twenty-five years ago to raise seedlings of the Wealthy, and devoted quite a portion of his farm at Excelsior to an experimental apple orchard. He originated a number of very excellent seedlings in the course of this work. Perhaps the most important and valuable of them all is a winter apple known as the Evelyn.

The Minnesota State Horticultural Society has made a special point of encouraging the raising of seedling apples and to this end offers liberal premiums for desirable seedlings exhibited at its winter meetings. The State Agricultural Society also offers liberal premiums for the same object. The Horticultural Society, in addition to these annual premiums, also offers $1,000 for a variety of apple that shall be as hardy as the Hibernal, as good a keeper as the Malinda, and of as good quality as the Wealthy. There are no limitations on this premium, and the officers of the society would be extremely glad of the chance to award it to any variety fulfilling the requirements, no matter what its source. The State Horticultural Society in conjunction with the Experiment Station has distributed to interested parties apple and plum seeds and seedlings and many experimenters have been thus interested in this line of work.

As the result of the work of the Horticultural Society and the Horticultural Division of the Experiment Station in en-

couraging the raising of seedling apples, it is quite common for fifty or one hundred plates of good seedlings to be exhibited at the September meeting of the State Agricultural Society. Some of these are of much promise.

The early settlers of this section found that the plums with which they were familiar in Europe, or in the Eastern States, were not hardy here, but that the native plums produced an enormous amount of fruit each year which was gathered in large quantities by the settlers. This fruit in its native state is perhaps one of the highest developed of any native fruit, and it was natural for the settlers to select the better trees from the woods and transplant them to their gardens. As a result of this work, and also of encouragement in the raising of seedling plums, we have now quite a list of plums that are well adapted for this section, and some of them are of very excellent quality and desirable for marketing. A new class of plums is now coming to us as the result of the combining of the native wild plum (P. Americana) with the dwarf Sandcherry (P. Besseyi). From this has come the so-called Compass cherry and a number of other fruits of considerable interest, and of probable value. The work of combining our native plum with the Japanese plums, to which it is closely related, has hardly been touched upon, but it seems to offer one of the most promising lines for experiment, and the Experiment Station of the University is putting a considerable amount of time on to the work of securing combinations of this sort. We think that from this work we ought to get plums of large size and good quality that are perfectly hardy, which will add very much to the pomology of this section.

We find that strawberries in this section are much more liable to be injured in winter than in the States farther east where the winters are milder, although when the beds are thoroughly protected here with straw they will produce heavy crops, and the Experiment Station is trying to obtain a hardier variety of strawberries than we now have, by crossing our cultivated kinds and some of the hardier forms.

The cultivated kinds of raspberries must be covered here in winter or they are liable to be severely injured. It is im-

portant to secure hardier sorts, and it seems as if it was probable that this might be obtained by crossing our better cultivated kinds on some of the native sorts, which are quite hardy.

The grapes of pure Labrusca parentage are often successfully cultivated in favorable locations in this section, but in severe locations and in severe winters they are injured in winter, unless laid down on the ground and protected, and even then the roots may be injured. It is desirable to secure a hardier kind of grape that will stand our winters, and be good enough in quality for general use. We have made something of a start in this direction by securing crossings between some of our cultivated Labruscas and our native *V. riparia,* and as a result have secured varieties that are perfectly hardy and extremely productive, and while the quality of these grapes is not up to the standard Labrusca sorts, yet they are a great addition to the general list of cultivated fruits suitable for our farmers.

What I have outlined will give you, in a general way, some idea of the problems that present themselves to the horticulturists of this section. The forms of fruit we now have have originated more or less by chance and largely through the sacrificing efforts of individuals who have profited very little, personally, from their labors. Our horticulturists have had in mind for some time the enlargement of the work of fruit breeding, and taking it up in a systematic way, and on a broad scale under suitable supervision. The matter was presented to our legislature last winter, and an appropriation of $16,000 was secured for the purchase of a farm suitable for this purpose. One hundred acres has been obtained and will be suitably equipped with buildings. On this tract of land we propose to carry on, but on a larger scale and more systematically, the work which has been done heretofore by individuals. We feel that we have had sufficient experience now in this line of endeavor so that we know where and how to work, in order to obtain the best results. We shall aim to keep careful records of the work done and to determine general laws underlying this work, which needs so much to be systematized.

The Chairman—Gentlemen, you have heard the valuable paper by Mr. Green. Is there any discussion about it?

Dr. Hansen—Mr. Chairman, I might say that in the work of combining the size of fruit of the Japanese plum with a native plum, we have made great progress. We had one this year that is excellent in qualities. It is as good as any plum we have ever eaten. As to the strawberry, we have been working a combination of native and wild strawberries with the best cultivated varieties. We have had fully ten thousand seedlings in that work and we have a great many varieties saved out of that vast lot—possibly over two hundred varieties—that we are working with still further, so that we have been able to secure strawberries in large numbers, that endure forty degrees below zero in winter. That is the test as to the hardiness. We never cover the strawberries. With the raspberry, we find in every respect the seedlings of all eastern varieties utterly failed, but the hybrids are perfectly hardy without any winter protection. Seven thousand seedlings have borne fruit and we have some excellent varieties and others coming on. Out of these seven thousand we expect the raspberry to be settled. We hope so at least. We have been working very successfully in that line, and I am pleased to see that work will be pushed in other States as well. It is certainly true that the only way we can get hardiness into our tender fruit is by crossing with species that are already hardy.

The Chairman—Yes, it would seem to resolve itself upon that point; our motto is, as in the Society of American Florists, "The art itself is nature." We assist nature, and I believe it resolves itself entirely upon that; we have to assist nature. We have to take the things we find at our hand, and with those do something. That seems to be the whole experience of everybody. You find your hardiest strawberries, and you take the better quality of strawberry, and cross.

Mr. Macoun—Mr. Chairman, this was brought out in Mr. Green's paper, what they are working for in the northwest, Minnesota, Wisconsin and the Dakotas. That is, Minnesota and Wisconsin particularly, which is the country of winter apples, they are working for a winter apple which will be good in quality and hardiness and will be productive, and they have offered one thousand dollars to the person who will bring forward a winter apple which will fulfil certain rules they have laid down, namely, that it will be perfectly hardy and productive, etc., and they have not got anyone yet to take the reward. We have been studying the situation a good deal at Ottawa, and have found certain facts in connection with the hardiness of trees, which we consider principles, which we are working on. In the first place, we find it is absolutely necessary to have an early maturing tree in order to get hardiness, and then we find in nearly every case, an early maturing tree means early maturing fruit, that is, fruit which ripens in the summer or autumn, which, of course, we do not want for a winter apple, but we find there are varieties which begin to mellow about November or early September, but which will hold up

until the winter, and which have a texture different from other apples which enables them to hold up during the winter, and we are working on those apples, recrossing those with other kinds. We find that practically all the winter apples which originated within fifty or sixty miles of our station, are of that character, that is, they are apples which mature early in the season, or which will keep all winter, under good cellar conditions, and that is the point I make where the weakness of planting and breeding has been in the past. We have been bringing them from hardy apples like the Dutchess in order to get winter apples, thinking that would give the winter breed from the Dutchess. Well, having winter breeds growing all around them, the chances are very slight. On the other hand, we have been breeding from the King, the Baldwin, and so forth, thinking we would strike a hardier apple from these hardy varieties, but the chances are very slight that we will, but what I think we ought to do is to take the seedling that we have, approaching winter apples as much as possible; they are natural crosses between the tender summer kind and the hardy winter kind. Then go on breeding from them; and that is what we are doing. I could name many others, the Milwaukee apple which originated with us, the Baxter, the Rufus, and several others which originated in Northern Ontario, which seem to fulfil all conditions, except in quality. On the other hand, there is no reason in my opinion, why we should not have the highest tender quality.

The Chairman—I think the time will come when we will get it.

Mr. Munson—I have a few facts which might as well be stated here as any other place. Data and the clear cut defined facts are the ones we wish more than any other, free from any theory. There is one thing that is well known. I have known in my own experience several times, in cutting plants of strawberries of several kinds, and taking them to Texas and transplanting them, that for the first season they are weak in resisting drought and heat; and the same variety (of course propagated from its runners) becomes hardier the longer it remains there. So we find this has been done; taking this from our plant that has been there several years, and getting plants from, say Northern Iowa and planting under the same conditions, we found our plant endured much better the first year or two.

The Chairman—You are getting acclimated.

Mr. Munson—The point I want to make is this, that there is a variation which has the power of resisting climatic influences. Variation takes place, and I wish to state the entire number of facts before I make the point. With reference to trees of various varieties, a species has always been tried, Texas grown, planted in the same orchard beside the seed of northern grown trees. I know of no exception. Invariably the northern grown trees come out several days before the southern grown trees of the same kind. Looking over the orchard, you will see that the others are dormant, and there is no difference that I can see, except that one is grown north and the other south, for several years.

It is this, that if there is a variation under climatic condition to any practical extent, add those little variations constantly for a long series of years, a number of generations, and may we not get a very substantial change, making a new adaptation for climatic influences?

The Chairman—I think that is what we will have to do. I think it is the solution of it. I cannot see anything else. I know it, and you know it too. You take our potatoes. We bring them from Maine to get them early. You take Early Rose and bring them from Connecticut. Your Maine potatoes will rot.

Secretary Barron—Speaking about potatoes, there is a problem in adaptation to climate, if you compare the American varieties with the European varieties. You take the American varieties of Solanum tuberosum across the ocean, and all through Europe they flourish amazingly; but you take the European type, the English varieties over here, and you are lucky if you harvest the weight of your seed. Dr. Hexamer made extensive experiments one year. He gathered in Europe every French, Irish and English variety he could get. He planted them and could get nothing at all. I have tried the same thing myself with European varieties and it utterly failed every time, yet I know for a fact the other side of the case is all right.

Mr. Macoun—We have some English sorts that have beaten our American ones. We have the Dalmeny Beauty. It is one of their early varieties there and it is doing splendidly. It is among the most productive potatoes we have. We have had it for twenty years. It is one of the best we have and resists blight. We have tried hundreds of varieties from Europe, and as a rule they are as you stated, but there are exceptions to the rule.

Mr. Munson—If you took the European varieties at almost the same latitude as those you have here, then you have made practically no change.

The Chairman—I was going to say that, but I think we all understand that very well.

The time has come for us to close, gentlemen. There are some announcements which the Secretary will make.

The Secretary then announced the titles of the additional papers on the program which would be taken up at the next session.

The Secretary also announced a complimentary excursion up the Hudson River, to take place the following day.

The Conference then adjourned to meet again at 10 A. M., October 3rd, at the New York Botanical Garden, Bronx Park.

SECOND SESSION.

Held in the Museum Building, New York Botanical Garden, October 3rd, 1907, at 10 A. M., the President, James Wood, presiding.

The President—Gentlemen, we are informed that there are quite a number of gentlemen on the grounds who will be in later, but time is going all the while and perhaps we had better consider some of the routine matters first.

The Secretary has quite a number of papers that have been contributed for the conference and he would like to ask your pleasure in regard to what shall be read.

Of course, the chief value of this conference is in the bringing out of these papers for publication. In no other way is it found possible to get such a mass of valuable material collected in one form and one publication, as in the proceedings of a conference like this.

If these are published in the bulletins of various institutions, they are detached and you cannot examine them collectively. You do not get the concensus of opinion on these subjects which you get individually, but in a publication of this kind, you can sum up and get the judgment of a great many able men all in one volume.

If the Secretary will please state what he has in hand, we will dispose of them now to save time. When others come in, we will consider other matters.

(The Secretary then read the titles of various papers he had received.)

Mr. Nash moved and Dr. Evans seconded, that these matters be referred to the editor for disposition. Motion carried.

The President—Now, Dr. Evans, will you be so good as to present your paper?

The following paper was then read by Walter H. Evans:

Experiments in Plant Acclimatization in Alaska.

By Walter H. Evans,

United States Department of Agriculture.

During the summer of 1897 the writer visited Alaska for the purpose of making an agricultural reconnaissance with a view to the establishment of one or more experiment stations in that Territory. At that time there was no agriculture and very little gardening in the country. As a result of that visit a central experiment station was established at Sitka, with branch stations at Kenai, Copper Center, and Rampart. The results of the endeavors to develop agriculture in Alaska have been published in the annual reports and other publications of the Office of Experiment Stations of the United States Department of Agriculture. The decision to recommend the establishment of the stations was based upon direct observations, a study of the native and introduced plants, and analogy between known conditions in Alaska and those in countries in Europe.

In searching for evidence that agriculture might flourish, the data found were very meager. During the Russian occupation some desultory attempts to develop agriculture were made at a few points, but the records left are conflicting and show such a lack of careful planning and attention to the experiments that but little could be learned from that source. The Russians did leave fairly complete data regarding temperatures and rainfall covering a period of fifty years or more, that proved of service in determining the available temperatures for plant growth. The settlers at the time of the visit were mostly engaged in trading, fishing, or mining, and little was to be learned from them. Recourse was had to a study of the native and introduced economic plants. Careful searches were made about a number of villages and lists were made of the introduced plants

that had survived and become established. Prominent among these were red and white clover, blue grass, and timothy. In several instances wheat, barley, and oats were found self-sown from feed or manure. In some cases these had made good growth and in some instances had ripened their grain. The few gardens existing about the towns were studied and additional data were secured. Most of the common hardy vegetables were found growing, although the varieties were plainly not of the best, and their cultivation was often neglected. Enough evidence was secured to warrant the establishment of stations that would more fully study and develop the agriculture of the region.

Among the first experiments planned after the establishment of the stations were some to test the adaptability of garden and field crops. Through the cooperation of the Bureau of Plant Industry of the United States Department of Agriculture seeds of a large number of varieties were obtained from northern Europe and elsewhere. These have been under observation at the several stations for some years and it is now possible to recommend varieties for planting that may reasonably be expected to grow and give adequate returns in average seasons. This has been of great value, especially to those who have small gardens about their homes. Formerly the seed supplies came from San Francisco or Puget Sound ports, and in many instances the varieties were not adapted to the more northern climate. A few specific results of the investigations may be of interest. With potatoes a large number of varieties have been tested, and for the past three years the variety Freeman has proved the best. In 1906 at the Sitka Station this variety yielded at the rate of 379 bushels per acre, followed closely by Gold Coin and Early Ohio. On the part of some varieties of potatoes there appears to be a tendency to a deterioration in quality after a few years' cultivation in Alaska, and investigations are in progress to determine its causes. Investigations have been carried on with cabbages, and the type represented by the Early Jersey Wakefield has proved the best for planting. The Drumhead and Flat Dutch types have almost uniformly failed at the station. Of peas the varieties Alaska and First and Best have given satisfaction and are now quite generally planted.

In addition to seeking for the best varieties of vegetables that were already grown, the stations have sought to introduce others, with considerable success. Kale, Brussels sprouts, Broad Windsor beans, rhubarb, cress, and various flavoring herbs have been distributed throughout the Territory and are being cultivated with marked success. In the gardens of Alaska all of the important hardy vegetables may be grown, and in some favored localities some of the more tender ones, as string beans, cucumbers, and tomatoes, are produced.

Considerable attention is being given to the introduction of hardy fruits, and about 12,000 fruit trees and shrubs have been distributed from the station nursery at Sitka. These consist of early maturing varieties of apples, crab apples, plums, cherries, raspberries, currants, gooseberries, and strawberries. Some varieties of all of these except the apples have already fruited at Sitka, and this year some of the apples bloomed and set fruit, but no report has been received as to their ripening. In connection with the fruit investigations, plant breeding work with strawberries, raspberries, and currants has been begun. In Alaska there are probably two distinct indigenous species of strawberries, one a coast species, the other occurring in the interior. Experiments have been under way with the coast form for a number of years. Plants were brought from Yakutat, where wild strawberries abound, to Sitka and were grown in rich earth for several years without fruiting. Upon transplanting them to poor, sandy soil they fruited abundantly. This species grows best in gravelly soil, is extremely hardy, and the berries are of excellent flavor. It has one serious drawback; the peduncles have the habit of strongly curving downward after fertilization, thus forcing the berries into the sand. Crosses have been made between this form and some of the best cultivated varieties, using the wild species as the staminate parent, and several hundred seedlings resulting from this hybridization are now under observation. Similar experiments have been begun with the smaller form that was secured in the interior of the country. Seedlings resulting from crossing the cultivated raspberry, which is frequently winter-killed, and the native salmon berry (*Rubus spectabilis*) are being grown and some should

fruit next season. Similar experiments are being made with currants and other small fruits.

The climate of the coast region of Alaska is insular in character and is distinguished by a heavy rainfall. In the interior the climate is continental, with less rainfall and higher summer temperatures. On this account investigations in grain growing are confined to the stations located in the valleys of the Yukon and Copper rivers. In the Copper River Valley early autumn frosts have destroyed much of the grain, but in no season has there been a complete failure to mature some portion of the crop. At the Rampart Station, which is situated in the Yukon Valley, some 350 miles from the mouth of that river and only about 60 miles from the Arctic Circle, cereals have ripened every year since the establishment of the station in 1900. For each of these stations the earliest varieties of cereals have been secured and from each of the more promising the earliest ripening heads have been gathered for seed. This procedure will be continued until local varieties are developed that are suited to the average season in Alaska. Last year three varieties of winter rye, one of winter wheat, three of spring-sown barley, and two of oats matured at Rampart, and a recent letter states that the grains this season are even better than last year, all varieties ripening except some common oats that were sown for hay. At each of the interior stations experiments with vegetables are being conducted along about the same lines as described for the work at Sitka.

In the work of acclimatization in Alaska the problems are twofold: to discover or develop varieties adapted to the moist coast region where the summer temperature is rather low, varying but little from day to day, and varieties for the interior where the growing seasons are shorter, the maximum temperatures higher, and the range of temperature much greater. In some portions of the interior the summer rainfall is deficient and that adds another factor to be considered. That some progress has been made is shown by the somewhat hasty review of the results of eight years' work. The greatest difficulty experienced in the Alaskan investigations is not due to climatic conditions, but rather to the ignorance or prejudice of certain individuals

who, comparing conditions in Alaska with those of the great Mississippi Valley or the Pacific Coast region of Washington, Oregon, and California, see nothing possible for Alaskan agriculture. Should the comparison be made with Norway, Sweden and Finland, which lie between the same parallels of latitude as Alaska, and where dwell more than 10,000,000 inhabitants, the contrast would not be so great and the possibility of agriculture along similar lines would seem more probable.

The President—The paper is now open for discussion.

Dr. Britton—I am interested in Dr. Evans' discussion of the conditions in Alaska. I would like to ask him in regard to the strawberries, if they both belong to the same genus.

Dr. Evans—I have not seen the one from the interior, but the one we have has seven leaves instead of five leaves. In the ordinary form of strawberry, there are five leaves.

The President—This paper of Dr. Evans' is very interesting to me, particularly for my sympathy with Alaska. I think it is a disgrace for the United States Government, that Alaska has been so shamefully neglected, and I hear with delight the work that Dr. Evans there has been doing in the establishment of an Agricultural Experiment Station, because it will lead to something more, and Alaska may be given, in process of time, by our Congress, a regular territorial government. The neglect of Alaska has affected all these interests in a great many ways. When we are considering the subject that Dr. Evans has given to us, we run right into Mr. Hick's field, of the thermal lines affecting the field of moisture. When this country purchased Alaska from Russia, we knew, if possible, less about it then than we do to-day, and the matter of the Pacific currents was not understood at all, and it was all that far-off region that nobody knew anything about, but the fact is that the Pacific current affects the climate of Alaska as the Atlantic current affects the climate of Europe and every part of Norway on the other side of the Atlantic, and we cannot judge by the latitude in this matter. I think, right along the line of discussion of this conference, it is possible to produce a variety of grains and of vegetables that will be adapted to that climate, or that latitude and that climate rather, and Alaska with its vast forest interests is worth a hundred times more than we paid for the whole territory. The gold mines and other mining interests are of very great value, and they should have an agriculture suited to their climate, as will assist all these other industries, that they may have community of interests there, without which you cannot have real development and progress anywhere. You cannot hang people on one peg or stand them on one stick. They have got to have foundations reaching around to every side, and this matter that Dr. Evans has presented is a matter of vast importance to that territory.

Dr. Hansen—Last fall I had the privilege of visiting Lapland, which is the northern section of Norway and Sweden, on behalf of the Department of Agriculture. I was exceedingly interested in the successful cereal cultivation, principally the raising of barley, and it is being done as far north as the Arctic Circle successfully. I hope some of these varieties will be a success in similar sections of Alaska. They raise barley and oats very successfully at 69 degrees and some minutes in some places in Norway. I was interested also in tracing red clover to its northern limit in Lapland. As near as I can get at it, in a state of nature, it is indigenous north of the Arctic Circle, the same as corn; it seems to be an elementary species, and is being selected by the Government in Northern Norway. One clover which I found exceedingly well suited to Northern Dakota was a form picked out by a peasant many years ago in the mountains, a perfectly smooth leaf, a form of red clover. It has lighter colored blossoms and no white spots on the leaves. We trust a few of that sort will be of value, so far as there will be no dusty hay. The hairs, I understand, make dust in the case of common clover. Several forms have been picked up in Siberia, forms of red clover, or closely allied to it, that were perfectly smooth in the leaf and very vigorous in habit. There is no reason, I think, why we should not extend cereal cultivation on this continent much farther north than at the present time, if we take advantage of the working of nature in adapting plants through the ages. As I said the other day, I believe to acclimate plants as we ordinarily understand the word, it is not the work for man to undertake. It is a work of twenty thousand years, but we can aid it by searching the world to find species that are adapted, and take them and take advantage of the work of nature through the work of ages, and then by taking advantage of hybridization of the forms, we may find it is possible to do a great deal.

Dr. Britton—I would like to ask Dr. Evans if he has an idea that the Great Indian fruit Quinoa would be of any value in Alaska, in case it could be grown there? Of course Quinoa is the fruit of South American natives right down to the coast, and I understand it is the staple food of perhaps eight million people where the cereals are not grown.

The President—High up in the Andes?

Dr. Britton—Oh, yes. I would like to ask Dr. Evans if there would be any economic utility in attempting that plant?

Dr. Evans—What is the name of the plant?

Dr. Britton—Quinoa.

Dr. Evans—I don't know. It is a case of educating the people, and we are having a hard enough time to educate them to grow some of their own food supplies. When they are perfectly willing to trade in some places and pay twenty cents a pound for potatoes when they could grow them themselves, and when they are willing to pay fifteen cents a pound for turnips—and I have seen turnips grown which would weigh from ten

to twelve pounds, and a single turnip would mean quite a little—when they are willing to pay those prices, it is hard to get them to do anything.

Now, in the interior of Alaska, there is probably—the Geological Survey has made an estimate and Professor Jordan has made an estimate —between fifteen and twenty thousand square miles up there that would be adapted for agriculture as is general in Northern Europe. But I was going to say that probably not one hundred acres all told are in cultivation, save perhaps in the vicinity of the new town of Fairbanks on the Tanana River, where they have found some excellent mines, and there is a rapidly growing town. At that place, a number of people have gone there, they are making truck gardens very profitable. We have this year opened a station between Chena and Fairbanks, and Professor Jordan says, he found early in August ripe wheat and oats and barley that had been self-sown, growing right there where there had not been any cultivation of the soil, and he sees no reason why it should not be done.

The President—Where did they come from, self-sown?

Dr. Evans—From manure and feed, right on a trail. That is where the seed came from. That is how it got there. These were not selected varieties. There were many things taken up there to feed the animals, and the animals had sown them. There had been no care given them, but a portion of them had ripened. They found a number of instances. He left Tanana in August, so they had plenty of time to mature. The valley is not a high one. It is undulating and it has a plentiful rainfall. Last year we had but 1½ inches rainfall during the entire growing season, and then Jack Frost came along and succeeded in putting to the bad about all our crops, but we went to work and cut them for hay, and sold this to the mail contractor at $200 a ton. That sounds like a very big price for hay, but we paid Indians $6 a day to help make that hay, and after we had sold this to the mail contractor, there was a very vigorous protest came through the Secretary of Agriculture from people who had a ranch along this same trail, and they protested in a vigorous way, as vigorously as they could, against the sale of hay by the Government to the mail contractor, and particularly, as they could not compete with the government selling hay at $200 a ton! (Laughter.)

Secretary Barron—In regard to the cabbages, you say it is the New Jersey Wakefield type?

Dr. Evans—Yes.

Secretary Barron—Have you any information about the varieties of the Little Pixie type? I wonder if they would do in that climate better than the larger headed varieties?

Dr. Evans—I don't know.

Secretary Barron—I know in some parts of Europe, the little cabbage grows much better than the big drumhead type, but over here, around New Jersey and New York, I have found that the Little Pixie type al-

ways becomes woody. I found it absolutely impossible to cook it into a tender condition.

The President—It is only a question of having a short season for development.

Dr. Evans—In the interior, yes. On the coast, they have a climate not as vigorous as here. The fall temperature at Sitka is between that at Washington and Richmond. Many people have an idea it is all snow and ice up there. Last winter, the lowest temperature—the lowest since we have had our station located there—was four degrees below zero. That is, the lowest temperature, at Sitka, has been since 1898, between this and ten degrees or more above that, but the long summer season has hardly ever a maximum temperature in Sitka along the coast region, of over 80 or 82 degrees. In the interior, at Copper Centum, we had maximum temperatures of 96½ degrees. We also had a minimum temperature of 70 degrees below zero, but there was a fairly good covering of snow, and it did little damage. In the Yukon Valley, where there is more moisture than in the Copper River valley, the snow has been sufficient to protect the winter sown cereals every year.. The snow falls ordinarily in October, and lays there until the latter part of April, when it goes off with a rush, and then there is a succession of twenty-four hour days of sunshine with an occasional rain to keep things watered until the middle of September, when things begin to meet with frost, and by the 1st of November, it is frozen up again, and there has nearly always been during the years we have been located at Rampart sufficient snow to protect the winter cereals.

The President—Of course, there is one thing that affects vegetation in the Arctic regions that we must always bear in mind, and that is, when you get twenty-four hours of sunshine in a day, you have got something that is affecting things in a most potential way. You could not get the vegetation that grows in the Arctic region without it.

Dr. Evans—I perhaps should have been more conservative in the matter of sunshine, but we do get twenty-two hours of sunshine and twenty-four hours of daylight, from the early part of June until late in July.

The President—We sometimes, those of us who go up in Canada, find the effect of sunshine which seems to us remarkable.

Dr. East—I would like to ask what the effect of such an amount of sunshine is on the plant growth, in relation to the amount of darkness?

Dr. Evans—I don't know, as far as our station is concerned. No investigation has been made along that line. There was one carried on some years ago, in connection with the station in Finland, but the result was rather inconclusive, but it is a subject well worth studying. We know very little about why it is that this longer time will have any effect, except that we suppose that if ten hours will produce a certain result, then twenty hours will do twice as much, but whether it does or not, we are not certain.

The President—Dr. Hansen, in your investigations, have you looked into it at all, the effect of a long day upon vegetation?

Dr. Hansen—As near as we can get at it, that is what saves time. In the long interior, the midnight sun is only for a short time, so that the plants do get a certain amount of rest during most of the growing season.

The President—It involves the question of the necessity of rest for plants, whether there is anything in it—do plants require rest?

Dr. Britton—We think the matter to be determined accurately, would have to be taken up in some such region as that spoken of by Dr. Evans. Of course, we could not determine that experimentally, except under the natural conditions. It would be impossible to attempt it anywhere else. So far as I know, there is no information on it.

The President—Can you give us the statement in regard to the period of twenty-four hours in which plants make their greatest growth?

Dr. Britton—I cannot tell you that.

The President—We will now ask Mr. Hays to read his paper.

The following paper was read by W. M. Hays:

Plant Improvements Needed in Specific Cases.

By W. M. Hays,
Assistant Secretary of Agriculture.

We have in the United States probably $5,000,000 worth of animal and plant products whose value could be increased 10 to 20% by breeding. The organization of public cooperative and private breeding establishments of sufficient magnitude to thus add $500,000,000 to $1,000,000,000 to our plant and animal production does not seem overdifficult. Effective methods have been devised and large results have been achieved which warrant that the plant-breeding work being organized in the United States Department of Agriculture and that being developed in the State experiment stations and in branch experiment stations be greatly increased. Seed farms, nursery farms, special plant-breeding farms and private individuals, both professional and amateur, have reason to increase their equipment and their energies along this line. Cooperation between the United States Department of Agriculture and the State experiment stations and the cooperation of these institutions with farmers and individual plant breeders and growers of purebred seeds and plants is developing rapidly. The American Breeders' Association with its forty-odd committees, meetings like this Conference on Plant Hardiness and Acclimatization, seed and plant breeders' associations, including associations for breeding specific crops, as seed corn breeders' associations, are doing very much to bring together in groups men to do team work in extending this vast enterprise.

The detailed studies of our soils and mapping of our agricultural regions according to soil, climatic and crop conditions are dividing up the territory of the American States into innumerable varietal districts. A thousand varieties of corn are needed for as many local conditions. There are scores, if not

hundreds, of local conditions requiring varieties of wheat specifically adapted to the respective conditions. Even in strawberries, where a variety like the Wilson may be successfully grown in twenty or thirty States, the work of the plant breeder is proving that the profits come from varieties adapted to special regions. All along the line our cereal, cotton and other field crops, some of which yield hundreds of millions of dollars; our fruit and vegetable crops, also representing vast sums of wealth, and even some of our forest crops, are ready to be made over to suit each local soil, climate, system of agriculture and market.

Until recently much of the plant breeding was done by amateurs. There was no organization to emphasize the greater importance of new values in crops representing immense wealth, and there was little systematic thought concerning the organization of our American plant breeding as an establishment of mighty import to our nation. The two first American commonwealths to begin the organization of State plant-breeding establishments were Ontario and Minnesota. Each of these States can show for the expenditure of forty or fifty thousand dollars in breeding field crops, products directly traceable to the work of plant improvement of forty or fifty million dollars. In no other of America's largest economic enterprises is there opportunity to make public funds so productive as in the improvement of our economic plants. Along some of the lines of least resistance in which improvements can easiest be made in crops of largest value, a dollar can be made to earn a thousand dollars and even in rare cases a million. It seems a fair and conservative estimate to state that our $3,000,000,000 worth of annual plant products can be changed by breeding alone into $3,300,000,000, at a cost of less than $3,000,000, one dollar earning on the average more than $100. Those who have had most experience in this line believe that the present organization of this work could be enlarged in ten years so as to be on the basis of expenditure and results last mentioned.

The breeding of corn throughout the upper Mississippi Valley, and for that matter throughout the entire United States, illustrates what is needed in the breeding of all our economic plants. Men in every State, in every group of agricultural

counties, in many individual counties and even on particular soils within counties, are breeding corn peculiarly adapted to their local conditions. For the most part varieties are being perfected so as to yield more of grain per acre, with some attention being paid to the quality of the grain. In some instances special attention is paid to secure more protein or more fat to give a grain with a higher feeding value or to suit the needs of some particular manufacture, as in the case of corn oil or wheat with stronger gluten. In other cases, the effort is to so breed varieties as to push the dent corn belt northward, to push the fodder corn belt northward, or to breed corn better suited to regions where droughts are severe. While in the middle West the varieties bearing one ear per stalk are believed to be best, in some sections of the South, where corn is planted wide apart, the evidence seems to be that breeders should produce corn with two ears per stalk. Some sections devote themselves to the production of seed of varieties peculiarly useful in distant regions, as for purposes of thickly growing silage and dry fodder for dairy cows farther north than corn for grain is profitable. One county in Missouri is said to have 10,000 acres of corn which has been bred for large, dense cobs for the making of cob-pipes. Popcorn suited to different localities is bred to expand larger in the popper.

In wheat breeding the production of varieties peculiarly suited to each local condition presents problems unique in themselves. In Minnesota, for example, with spring wheats, the effort is to produce sorts which will be so highly rust-resistant that this disease will not be able to reduce yields 10 to 60% annually. In the case of winter wheats for Minnesota the need is for varieties bred to greater hardiness that they may endure the winters often not well covered with snow and very cold, and thus carry northward the larger yielding ability of this class of wheats. Durum spring wheats which yield well need to be so bred as to have gluten of tougher quality and the grains to be made less flinty, so as to be more easily ground into high-class flour. There is need of hybrids of these three varieties of wheat, taking out of each parent kind and combining into new varieties the desirable qualities of the several parent varieties. Improved

classes of wheats are needed for the black prairie soils of southern Minnesota, for the sandy soils of northern Minnesota and for the very fine clay soils of northern Minnesota, also the very fine clay soils of the Red River Valley, that the yield for the State be raised from fourteen bushels to twenty bushels. In Kansas varieties are needed which are hardier in winter, varieties whose chaff more tightly holds the kernels so that the grain need not be harvested the day it is ripe; also varieties which will thrive under the drouthy conditions of the western part of the State. In Washington and surrounding States varieties are needed which combine hardiness or ability to live over winter with high yielding power and ability to stand erect and not shell out for days or even weeks after ripe until harvested; with higher content of superior gluten which will otherwise increase the value per acre of this crop on distinctive soil and climatic areas, as in the Willamette Valley, in the Yakima Valley, in the Sacramento Valley, and in numerous other localities distinctive in soil, climate and in the system of farming into which the wheat must adapt itself.

Commercial apples need to be bred not only for the great apple regions of New York, Michigan and Missouri, but for every region in the country where it is practicable to raise apples for family use. Where we have hundreds of successful varieties we could have thousands, that we might better adapt varieties to localities and also that we might throw away many that are not now adapted to the regions in which they are grown.

The breeding of plants is a long-time proposition. The elements of the work are men with a genius for creative breeding, the unit characters to be blended into new varieties or species, and means and organization for long-continued efforts. We need to develop a class of highly trained breeders who, through long and extensive experience, will become highly efficient in turning public and private dollars into double eagles, and even pennies into dollars.

The work of men like Mendel, DeVries, Bateson, Davenport, Castle, and Webber in discovering the laws of heredity is of immense benefit, and for generations men of this class will continue to add to our knowledge facts of large practical value

in the improvement of our plants and animals. Their work should be supported most liberally, that they may rapidly learn the needed truths useful to those who create new economic and artistic values. Of no less value is the work of men like Vilmorin, Burbank, Neilson and Garton, who produce new values which give not only inspiration to breeders but which lead legislative bodies, firms and individuals to invest the necessary sums of money to reap the full possible profit represented by the hundreds of millions mentioned above. If the faith of plant breeders is truly placed—that every agricultural district would respond to varieties especially bred for its conditions,—we have only touched the fringe of the possibilities for creating wealth by breeding plants.

All the students of theory and all the practical creative breeders, so far as I know, believe that in each species there is one plant among very many which has peculiar breeding power, peculiar projected efficiency, peculiar variety-forming values along needed lines, and that the bulk of the everyday work of breeding is in finding these "Shakespeares of the species."

The work of creating still greater Shakespeares of the species through hybridizing may prove to be the more important, as it is the more interesting; but the bulk of the expense of our needed Federal, State and private breeding establishments must be in the work of "mingling art and statistical methods" in ferreting out the occasional individuals with peculiar value, segregating their "blood," and in giving them a chance to prove themselves adapted to increasing production in broader or lesser areas.

The efforts of the American Breeders' Association through several dozens of committees and sub-committees to secure team work in the making of plans for breeding each species of plants and animals is beginning to bear fruit. There is value in friendly rivalry, and men who, like Mr. Williams in his statement of methods for breeding corn, are securing the applause of their fellows, are appreciating the recognition for public service well done. The making and the execution of specific plans for breeding each species so that it will better serve in its present habitat, and will be adapted to habitats which it cannot now quite

succeed in, are as much questions of the hour as the discovery of additional limitations to Mendel's law, or as are other general questions of the theory of heredity.

The President—Any remarks to be made on Mr. Hays' statement?

Dr. Hansen—I would like to ask if you met with any objection by the nurserymen?

Mr. Hays—I would like to say that I heard one of our leading nurserymen say that he did not know anything that might help the nurserymen more than this, because they only shipped a small number of plants, as many of them were often taken up by growers who never grew before, and our nurserymen stand right by us. We do not mean to antagonize them and the nurserymen seem to have no objection. Of course, they are all looking for orders for this plant, but we select from some nurserymen whom we know to be reliable, and whom we know by experience will put up a plant as we want him to. At first, we tried it by tender, but at last we got down to one man who does the work for us. There is no objection whatever on the part of the nurserymen to the work. In fact, it has been a help.

Mr. Southwick—I would like to call the attention of the gentlemen present to the educational value of this work. That is very important, the educational value of this work, to the people of this province or any section.

The President—The subject is one which has great economic value and has a bearing also upon the subject of hardiness and acclimatization. We would ask Mr. Hicks now to present his statement on "Plants from East Asia and Western Europe on Long Island."

The following paper was read by Henry Hicks:

Plants from East Asia and Western Europe on Long Island.

By Henry Hicks, *Westbury, L. I., N. Y.*

Foreign plants will succeed best in the vicinity of New York City if from regions of equal or greater annual variation of temperature and a similar January mean temperature or isotherm. They should also be from a region subject to moderate drought, but not from a desert region.

The January average of New York City is about 30° F.; the July average 70°; the annual range is therefore 40°.

The countries where these two lines pass are northern Japan, Caspian Sea, Caucasus Mountains, southern Russia, Austria and eastern Germany. These two lines cross here and in northern Japan and plants from there thrive best here.

In Colorado and Korea the line of 30° January average is crossed by 50° equal annual range, and conifers from there and northeast China are not injured by our extremely severe winters even when our native conifers are injured, because they are accustomed to a more severe or widely and suddenly variable climate than our natives. The Colorado conifers are subjected to a more brilliant winter sun than ours, therefore they show no damage from the winter sun which burns our pines, hemlocks and arbor vitæ. The Colorado conifers are accustomed to a more severe, dry winter wind when the frozen ground does not supply moisture, therefore they are not killed back as occasionally happens with the hemlock, red cedar, white pine and arbor vitæ. But in a hot June the tips of the new growth of the Colorado evergreens is killed. This trouble I have not seen described. It does not affect the conifers of Korea and Manchuria and northern Japan, and they are the best foreign conifers.

Evergreens from the Caucasus, from the Taurus Moun-

tains in Asia Minor and from Austria and the Balkan Peninsula thrive here. That region has an annual range of 30 to 40° and a January average of 30° or a little below.

Evergreens from the mountains of North Carolina thrive here. Linville, North Carolina, has 31° January mean, and about 40° annual range.

Evergreens from Maine, the Adirondacks and Michigan thrive here, except a few slightly dislike our warmer periods in winter, alternating with cold, dry northwest winds. In other words, they do not like to be awakened in winter by bright sun and a temperature of 65°. They like steady cold.

The above applies to regions whose conifers thrive here. Evergreens from the two west coasts do not permanently thrive here.

The line of 20° annual range passes through Spain, France, England and Norway, and through California, Oregon, Washington and the coast of British Columbia, the southern slope of the Himalayas and southwestern China.

Isothermal lines and latitude are not a guide to the introduction of plants from the two west coasts. The isotherm of 50 passes through here and through southern England, Ireland and the State of Washington.

Prof. W. M. Davis, of Harvard, from whom this map is copied, says that the reason for the small range of temperature along our Pacific coast and the western coast of Europe, and the area of strong range along our eastern coast, and the eastern coast of Asia, is the combined action of ocean currents and the winds, particularly in the control of the distribution of temperature by the winds.

In temperate latitudes the prevailing course of the winds is almost from west to east.

The above statements have been made mostly with coniferous evergreens because they are awake all the year and record the whole climate of their native country and where introduced, except drought. They generally require only a fraction as much water as deciduous trees.

Deciduous trees and shrubs go to sleep in winter by dropping most of their evaporating surface, and if their foliage is

damaged in summer it is born anew the next spring. The evergreens are more severely handicapped by their injuries.

With few exceptions deciduous trees, shrubs and vines from western Europe are not happy here. They suffer more from fungus diseases than our natives, and the bark and trunk are more liable to crack when the temperature goes below zero. The foliage often feels softer, less leathery, less able to stand drought, darker green and arranged more at the ends of the branches so that looking up into the trees is as into a hollow dome.

Most of the fruits brought from Europe to the eastern coast of the United States is less healthy than the native trees. Fortunes have been spent trying to introduce fruits that failed. We hear of these European fruits thriving better on the Pacific coast.

There should be a series of experiments along the east coast of the United States to introduce the economic and ornamental plants of eastern Asia and other regions of similar climate, and carry on extensive plant breeding with them and with the native plants and the more highly improved plants of Europe.

The above conclusions have been reached from observations on plants mostly growing on Long Island, northern New Jersey, New York State, the vicinities of Boston, Philadelphia, Washington and Norfolk, Virginia. Long Island has been probably the oldest and most extensive testing-ground for European and Asiatic plants. From the Prince Nursery or Linnean Botanic Garden were distributed many foreign trees in Colonial days. Later the Parsons Nursery introduced the Hall and the Hogg collection of Japanese plants. The Charles A. Dana collection is on Long Island. Nursery stock imported from Europe and Japan has been very extensively used.

Dr. Asa Gray's discovery of the close relation of the floras of eastern Asia and eastern North America led me about seventeen years ago to begin noting the behavior of those plants here. At that time we were importing a large part of our young nursery stock from France and Scotland. Later I imported most of the species offered by the German, French, English and Japan-

ese nurseries, and collected seed of native species. The latter is the predominant policy at present.

These conclusions have been corroborated by Prof. C. S. Sargent in "Notes on Cultivated Conifers," in *Garden and Forest,* October, 1897, and "Classification of Climates," by W. Koppen, in *Bulletin American Geographical Society,* August, 1906.

There are many exceptions to the above conclusions. Many Japanese plants fail to be hardy in severe winters, many suffer from summer drought and some have bark killed in winter. This I assume to be due to the more equable and humid or oceanic climate of Japan. Some Japanese plants are severely attacked by San José scale, as Japanese quince and Japanese plum. The San José scale is severe here only on *Rosaceae* from humid or equable climates.

From western Europe several trees thrive here, as the beech and Norway maple.

The President—Mr. Hicks' paper seems to me to be of very great interest. It is now before the Conference for discussion.

Dr. Hansen—I believe this conference can bring out some fundamental truths and conditions. It is a conference on Acclimatization of Plants. If we can bring out evidence enough to show that acclimatization of plants is an impossibility in human hands, it is the most fundamental thing that has ever occurred in American horticulture. I want to come just as near making that a positive statement, that acclimatization is an impossibility for human hands, as I can; that is, by selection alone—that is, to adapt a plant to a greater degree of heat or cold by selection alone—that is an impossibility, and that is the platform that fundamentally affects all our experimental work. In the same way, it might be true that acclimatizing plants from the far north to the far south or from a humid section to a dry section, or vice versa, is an impossibility. Here is something that goes to bear out that truth, that for several years past we must take what Nature gives us and not attempt to perform the work of twenty thousand years in a generation.

The President—We have had the fond theory that we could cooperate with Nature.

Dr. Hansen—Yes, by hybridization, but not by acclimatization.

The President—In relation to the statement by Mr. Hicks, that our native plants, trees, etc., are not only more vigorous in various ways, the contrary also seems to be true when he says that the foreigners are more subject to diseases than to enemies. Now, I have noticed this year for the first time, that my English oaks are badly infested with scale. Some

fine specimens have been destroyed. I have never seen any scales on our native oaks. The royal oak was the oak that saved his Majesty when he took refuge in the royal oak, and it was destroyed this season by the scale. Of course, one swallow does not make a summer, but so far as it goes, it is in line with his remarks. I think the practical value of these observations that Mr. Hicks has presented to us is very great.

Prof. Munson, of West Virginia—Relative to the remarks of Dr. Hansen with reference to the change of character of a plant without hybridization, it is very generally attempted, among horticulturists, who believe, for instance, that a peach grown in Michigan will stand a much lower degree of temperature, than will a peach, the same variety, if it was grown in Alabama, though of course, the propagation of peaches in Alabama is only by florists; but a peach grown in Alabama would be killed by a climate that would have no injurious effect upon it whatever if it was grown in Michigan. It would seem true that there is a distinct modification of the character of the individual.

Dr. Hansen—Commenting on that, I believe it is a fact well known to nurserymen that there is such a thing as trees being too soft for certain soils, and therefore a peach tree raised farther north, for instance, would be too hardy for Texas, hardier than one raised on the Gulf Coast, for instance, and this temperature effect did not save the peach orchards of Michigan last winter. There were hundred of thousands of peach trees killed last winter in Michigan. I do not wish to discourage, by any means. I just throw it down as a sort of challenge to be picked to pieces by all of you who so desire. I do not wish to discourage any effort at importation. In fact, I have had a little to do with that sort of thing myself, but the thing to be observed in this matter is that we must study ecology more than we have. I am pleased to see Mr. Hicks has done so much in that direction. We must study the climate in various parts of the world, as a guide to our importation work. An attempt to acclimate a plant that is much softer is a rather difficult task, whereas, if we pay some attention to the native habitats of plants in making our selection, we will save an immense amount of money. That is the only point I wish to make, but in addition to that I stand here to say that we can acclimate plants by hybridization, that is, carrying over this inherent hardiness, whatever it is, and helping it by heredity.

The President—In following the statement of Prof. Munson in regard to the peaches grown in Michigan of the same variety being hardier, even though cultivated from the bud, it calls to my mind that the hardiest peach stocks in America are on the elevated lands of Tennessee and nurserymen send to Tennessee to get the seed. It is a great business, the peaches grown at that high elevation, which means northern climate. Now, why are those peaches grown there and have been grown there for we do not know how long? The question whether they were not introduced from Mexico and got there in that way so that they are more thoroughly Americanized and acclimatized than any other

peaches in the Continent, is something we cannot decide. There are wild peach forests in Wisconsin and they could only have got there by the Indians receiving them from the Mexican settlers about four hundred years ago. Now, if these peaches have become as near indigenous as it is possible for a foreign thing to come to in Tennessee, and have got stamped on them the requirements of the American continent—if that is the case, as I believe it is—why, it is the hardiest peach stock in America and in the world, outside of Manchuria. Doesn't it conflict then, with Dr. Hansen's claim? Now, so far as I know, no other reason is advanced for the extreme hardiness and vigor, so that nurserymen avail themselves of it, of this peach. stock of Tennessee, in this high elevation. There is no reason for it except the fact that that elevation gives it not longer than the historic history period—unless it is in the wild peach of Wisconsin—I would like to have it explained.

Dr. Hansen—In answer to the question: I would say I have tried some of those hardy peach stocks and also the common peach pits. I have had the hardy peaches of Iowa that they have been raising for years from the hardiest stocks, and after a hard winter, I never discovered any difference. One might be deader than the other—they were both dead! I have had the French crab from the side hills of France, and I have had the Vermont plant seedlings, and those from Vermont are supposed to be the hardiest on the American Continent, but the same observation could be applied to both; they were both dead after a hard winter.

Mr. Macoun—Gentlemen, I regret very much I was too late to hear this paper, because it is a subject I am very much interested in, but it seems to me the question Dr. Hansen has brought up is a very complicated one. The question of mere temperature alone, I think is a small factor —not exactly a small factor, but it is only one of the factors regarding hardiness. For instance, I understand experiments have been carried on at places, among them, the Department of Agriculture, Washington, for testing the seeds at different temperatures. Some were submitted to very low temperatures and they survived the temperature. It seems to me that the question of humidity, ripeness of wood and various other causes also influence the hardiness of plants, and when we introduce plants from over the seas, we do not know exactly the way they lived there. We do not know whether they are woodland species or not. They may be a species that thrived in very different conditions. We found in our experience at Ottawa, with our own native hemlock, when we transplanted it from woodlands where it thrived at home, that it is very difficult to get it to thrive in the open, and in the case of four or five other trees, I think they have shown a great deal more vigor than some other species which correspond with them. There is the northern species P. excelsa. We have no other tree which has a more rapid growth than the Norway spruce, and no other which is less subject to disease. Then there is the European which is much stronger than our own species, but I don't

think it is much more subject to insects than our own. We also have the Scotch Pine which is a hardy tree. There is also the Norway maple which is an exceedingly rapid growing tree. Now, in regard to fruit, there is the question of protection, bending down and covering the soil—but the mere protection of other trees is a very important factor in influencing hardiness; and I know in Manitoba, where it is very difficult to grow apple trees, some of our most successful nurserymen grow the trees between rows of celery. I suppose this protection which they might find in no other place, has enabled them to grow these trees.

Dr. Britton—Mr. Chairman, as having perhaps a remote bearing on this subject, I would like to call your attention to the Century Plant in the Island of Jamaica, from which I have just returned. That is a species which is in Jamaica, very abundant, in all the arid southern portions of the Island. Now, that plant has a diametric range of five thousand feet in temperature. That is, it extends right from the shore on the dry side of the island right up to the tropical station on the mountain side towards the south, and that evidently is subject—you see the same species is there—to all the variations from the highest to the dryest kind of tropical temperature up to the temperate zone, which is the temperature at Cinchona, because Cinchona is one of the most delightful places for residence in the world. It occurs right on those mountain sides, apparently not affected by temperature, but apparently influenced by humidity. This is an example of the species which goes according to the humidity, but does not seem to care very much about temperature. As bearing also on the subject, in a less degree, there is the same history in the case of Jamaica in the Pilocereus. The species which I found in other lands, is the same. I have found it in other places just the same as this. It grows on the south side to a height of two hundred feet, higher, in fact, than any other species of Pilocereus in Jamaica that I have observed. Instances might be multiplied, I am sure, showing the great latitudinous range on the south side of the island, apparently regardless of conditions of temperature.

The President—It has been suggested that the most profitable way to spend the afternoon would be to look around the garden and see specimens growing here that would illustrate many of the things that are under discussion at our meeting.

It was then moved by Mr. A. L. Willis, seconded by Prof. N. E. Hansen, that the thanks of the Conference be given to the American Institute, and to the New York Botanical Garden, for the courtesies accorded them.

There being no other business before the Conference, adjournment was then taken.

Papers Read by Title

Cooperative Methods of Ascertaining Hardiness in Fruits.

By Prof. H. L. Hutt,

Ontario Agricultural College, Guelph, Ontario.

Hardiness is largely a matter of locality. In speaking of any particular fruit we may say it is hardy in a certain district, although it might be quite tender in another. For this reason, the determination of hardiness of any kind of fruit is more or less of a local problem and cannot be ascertained at any one experiment station for all parts of the country. This question of hardiness and adaptability of the various kinds of fruits to the different sections of the Province was one of the problems which confronted the Ontario fruit-growers a number of years ago, and has been more or less definitely solved by cooperative methods during the past ten or twelve years. When we began this method of testing we already had two Government Experiment Stations, one at Guelph and one at Ottawa, where extensive tests were being carried on with the fruits that could be grown in these localities; but in addition to these, fourteen prominent fruit-growers were selected in as many different parts of the Province to carry on experimental work in the testing of fruits most largely grown in their districts. These Fruit Experiment Stations were not purchased by the Government, but were left in the hands of the private owners, who were furnished with large collections of varieties of the fruits most grown in their locality upon which they could make careful observations and report results.

This cooperative experimental work is under the management of a Board of Control, composed of the President and Horticulturist of the Ontario Agricultural College, the Horticulturist of the Central Experimental Farm at Ottawa, and three representative fruit-growers appointed by the Provincial Fruit Growers' Association.

In the selection of the experimenters men were chosen who

had the confidence of the growers of their district and who already had under cultivation more or less extensive plantations of the various kinds of fruits, and were thus prepared to make reports from the very first upon the varieties they already had in fruiting.

Each experimenter makes a full report each year to the Secretary of the Board of Control, who classifies and prepares the reports for publication.

As a result of all this testing during the past twelve years, Bulletin No. 147 was published last year, giving lists of the fruits recommended for planting in the various parts of the Province. This little bulletin of ten or twelve pages contains in a condensed form information which is of incalculable value to the fruit-growers and farmers of Ontario, because it is a reliable guide to planters, and is well worth all the money that has been expended upon the Fruit Experiment Stations.

The cost of this work has not exceeded $1,800 per year, and this has been expended principally for the purchase of trees and plants for testing; for the annual allowance, varying from $50 to $200, paid to the experimenters for their reports, and cost of publication of the same.

As an outcome of this work, the Department of Agriculture has just published a beautifully illustrated and descriptive volume on the "Fruits of Ontario." The descriptive work has been done largely by Mr. L. Woolverton, Secretary of the Board of Control, and has been carefully revised by the members of the Board. It is expected that it will be a standard for reference for Ontario fruit-growers for many years to come.

For fuller information regarding this work, I need only refer to the Annual Reports of the Fruit Experiment Stations published by the Ontario Department of Agriculture, which are distributed free upon application.

Another phase of cooperative experiments in fruit-growing which has been productive of great good in Ontario, is that carried on by the Horticultural Department of the Ontario Agricultural College. This work is conducted through the agency of the Experimental Union, an organization managed by the officers, students, and ex-students of the College, but every resi-

dent of Ontario interested in horticulture is invited to join in the work and benefit by the results of the experiments. This cooperative testing has been in progress many years with farm crops. The work with fruit-growing began fourteen years ago with sixty experimenters, to whom were sent small collections of strawberries, raspberries, or currants for testing. The following table gives a good idea of the scope and progress of the work, as it shows the nature and number of experiments undertaken since its inception in 1894:

COOPERATIVE EXPERIMENTERS.

Nature of Experiment	Number of Experimenters														Total
	1894	1895	1896	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906	1907	
Strawberries . . .	15	20	20	50	100	100	100	116	143	119	166	222	244	713	2,128
Raspberries . . .	15	20	20	20	25	25	25	35	48	44	46	69	93	206	691
Black Raspberries	15	20	20	20	25	25	25	35	52	43	53	46	67	185	631
Blackberries . . .	—	—	20	20	25	25	25	35	32	47	42	42	32	72	417
Currants	15	20	20	20	25	25	25	38	48	29	39	48	31	85	468
Black Currants . .	—	—	—	—	—	—	—	—	—	43	47	50	53	110	303
Gooseberries . . .	—	20	20	20	25	25	25	50	47	39	40	55	71	137	574
Grapes for Southern Ontario . .	—	—	—	—	—	—	—	—	—	—	—	—	82	178	260
Grapes for Northern Ontario . .	—	—	—	—	—	—	—	—	—	—	—	—	69	96	165
Apples for Southern Ontario . .	—	—	—	—	—	—	—	—	—	—	—	—	144	281	425
Apples for Northern Ontario . .	—	—	—	—	—	—	—	—	—	—	—	—	264	329	593
Total . . .	60	100	120	150	225	225	225	309	370	364	433	532	1,150	2,392	6,655

From this it will be seen that a total of 6,655 lots of plants have been distributed for testing. These experimenters are so scattered that they are found in every township and district in the Province.

We may briefly outline the method by which this distribution is carried on. Early in the year a circular is distributed, and announcement is made in all of the leading papers of the Province that plants will be given free of charge for testing, on the understanding that each experimenter who receives the plants will follow the instructions furnished with them, will carefully look after the plants, and will report each year upon the yield and growth of the plants upon blank forms furnished annually for this purpose.

The following is a list of the experiments, showing the class of fruits and varieties of each offered for planting. These varieties make a good general collection for home use and in most cases cover the season from early to late. They have been selected after years of careful testing at the various Government Fruit Experiment Stations as the ones most likely to give good results throughout the Province.

Experiment No. 1. Strawberries—Splendid, Fountain, Ruby, and Parsons—12 plants of each.

Experiment No. 2. Raspberries—Cuthbert, Golden Queen, Marlboro', and Columbian—6 plants of each.

Experiment No. 3. Black Raspberries—Gregg, Kansas, Palmer, and Older—6 plants of each.

Experiment No. 4. Blackberries—(Adapted only to *southern sections of Ontario) Agawam, Eldorado, Kittatinny, and Snyder—6 plants of each.

Experiment No. 5. Currants—Fay, Red Cross, Victoria, and White Grape—2 plants of each.

Experiment No. 6. Black Currants—Champion, Lees, Naples, and Black Victoria—2 plants of each.

Experiment No. 7. Gooseberries—Downing, Pearl, Red Jacket, and Whitesmith—2 plants of each.

Experiment No. 8. Grapes—(For *Southern Ontario) Concord, Wilder, Niagara, Lindley, Brighton, and Vergennes—1 vine of each.

Experiment No. 9. Grapes—(For *Northern Ontario)

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*This division of the Province into North and South may be approximately made by a line running from Collingwood to Kingston.

Champion, Worden, Winchell, Delaware, Lindley, and Moyer —1 vine of each.

Experiment No. 10. Apples—(For *Southern Ontario) Primate, Gravenstein, McIntosh, Blenheim, Rhode Island Greening, and Northern Spy—1 tree of each.

Experiment No. 11. Apples—(For *Northern Ontario) Transparent, Duchess, Wealthy, McIntosh, Scott's Winter, and Hyslop Crab—1 tree of each.

The plants for this distribution are purchased from nurserymen who make a specialty of growing good plants and putting them up in good condition for distribution by mail. We are obliged to make use of the mail, because in many cases the experimenters live so far from express offices that it would not be practicable to send the plants in that way. Applications for plants are filed in the order in which they are received until the appropriation for the purchase of plants is exhausted. When sufficient applications have been received to make up the lists of those to whom plants will be sent, circulars are sent acknowledging receipt of application and informing the applicant that plants will be sent by mail in proper time for planting. Special directions are also furnished for conducting the experiment with each kind of fruit and blank forms are furnished upon which to report results at the end of the season.

For a copy of these cultural directions for the various fruits and more general information regarding the work, we must refer our readers to the last Annual Report (1906) of the Experimental Union.

A record is kept in the office of the Horticulturist giving the name and address of each experimenter, the kind of plants sent him for testing, and a brief record each year of his report upon the same. Naturally, many experimenters who receive plants fail to report after two or three years, although there are many who have been engaged in this work almost from its beginning and have been sending in regular reports. In this way we have a list of careful experimenters all over the Province,

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*This division of the Province into North and South may be approximately made by a line running from Collingwood to Kingston.

whose results are of value, not only to themselves, but to those in their neighborhood.

Upon receipt of the reports at the end of each season, they are summarized and a general report of the work is presented each year at the Annual Meeting of the Experimental Union held at the College usually during the first week in December.

This work is of great educational value to those engaged in it, and the greatest value naturally accrues to the individual experimenters who receive plants and carefully conduct the experiments. It is valuable also because it affords a means of distributing the leading varieties throughout the Province, and many are thus given a start in fruit-growing who had never before given it any attention. The educational value, too, of the cultural directions furnished with plants is helpful, enabling growers to adopt the best methods in their fruit-growing. This work, although conducted on a scale calculated more to help the grower in his supply of fruit for home use, is also having a marked effect on the commercial fruit-growing of the Province.

Similar work was begun this year with vegetables, and seeds of a few of the leading varieties of beets, carrots, lettuce, and tomatoes were distributed to about fifteen hundred experimenters. Reports upon this work are now coming in, and one of the most striking features in connection with it has been the eagerness with which it has been taken up by the various schools of the Province, where school gardens have been instituted, and probably in no other place could it have a greater educational value.

Factors Affecting Hardiness of the Peach.

By U. P. Hedrick, *Geneva, N. Y.*

The peach affords a striking example of a plant undergoing acclimatization. In the wild state, this species is endowed with a constitution fitted to endure the heat of climates almost subtropical. Under domestication it is gradually becoming inured to climates far to the north of its habitat and so cold that at first it could not have lived in them. It may be that this change is somewhat due to the acclimation in which the plant is naturally or spontaneously becoming habituated to cold; but the peach can now grow in colder climates than formerly, chiefly because of the efforts of man to secure this change in the species. What are the means by which man can aid in acclimatizing a species or a variety to a climate at first injurious to it?

I have made two efforts to find some explanation of the varying behavior of peach trees during freezes and frosts, working at the problem from the standpoint of the horticulturist, and the information obtained in these investigations shows some of the means by which man is helping to acclimatize the peach and by which possibly other species might be acclimatized. In the spring of 1905 I addressed letters to about one hundred of the best peach growers in Michigan, asking for their experience as to the hardiness of the peach in tree and bud. In the spring of 1907 about the same number of letters were addressed to peach growers in New York. This paper is a brief review of the answers obtained. In making these investigations I have visited the orchards of many of my correspondents, and have noted the condition of the trees under consideration and have a personal knowledge of many of the conditions discussed.

The factors considered in the investigation fall under two heads:

I. Cultural treatment, which increases the ability of the individual trees to withstand cold.

II. Variations in the species favorable to greater hardiness to cold.

In presenting and discussing the information obtained, I shall advance few or no theories but shall simply set forth the facts that have been reported to me.

I.

The factors of environment and of cultural treatment noted as affecting acclimatization are as follows:

INFLUENCE OF SOIL ON HARDINESS.

It is usually held that trees are hardiest on sandy, gravelly or stony soils. In the peach orchards of Michigan the growers consulted held this to be the case almost without exception. But in New York the kind of soil seems to make but little difference, providing it is warm and dry. If these two factors be favorable peaches seem to thrive in any of the soils of New York. The difference in opinion between the peach growers of Michigan and New York arises from the fact that the great belt in which peaches are grown in the first-named State has a sandy soil, and growers there have scarcely tried the peach on clays, loams or shales upon which some of the best orchards in New York are located.

But this point is made clear: *the peach must have a warm, dry soil* to secure the greatest possible hardiness inherent in the species. Only in such a soil can trees make a strong, firm, well-matured growth that seems to be conducive to hardiness.

Many growers in both States speak of the desirability of a gravelly subsoil to secure a hardy tree. Such a subsoil seems to be conducive to the warmth and dryness of roots and it is probable that so far as hardiness is concerned it matters little whether this subsoil be overlaid with sand, gravel, loam, clay or combinations of these.

DOES THE AMOUNT OF MOISTURE IN THE SOIL IN WINTER AFFECT THE HARDINESS OF THE PEACH?

The evidence as regards this point is clear. Either extreme of moisture—excessive wetness or excessive dryness—gives

favorable conditions for winter-killing. A wet soil is conducive to sappiness in the tree and also freezes deeply. Severe cold, especially alternating with warm weather or accompanied with dry winds, causes evaporation of water from trees, and if the soil be so dry as not to furnish moisture to replace the evaporated water, harmful results ensue. Several experiences were given in Michigan in which trees were injured far more from winter freezes in a dry than in a wet soil. The statement was made by several growers that twigs and buds which are more or less shriveled in winter from lack of water or lack of maturity are almost invariably winter-killed.

WHAT EFFECT DO FERTILIZERS HAVE ON TREE GROWTH AND HENCE ON SUSCEPTIBILITY TO COLD?

It has always been held in theory that fertilizers with any considerable amount of nitrogen, as barnyard manure, cause trees to make a heavy, rank, soft growth susceptible to freezing. The majority of the peach growers consulted in this investigation still hold that such is the case, but a very considerable number of them, among them some of the best growers in the two States, hold that trees are more likely to suffer from cold if underfed than if overfed. Their experiences indicate that vigorous, vegetable growth in early summer can be made of great service in counteracting cold and that half-starved trees, or those which have been allowed to bear too heavily, are apt to suffer most from freezing. Fertilizers properly used do not, in the experience of these growers, necessarily induce a rank, soft growth. By using properly balanced fertilizers, by stopping cultivation at the right time, and by judicious pruning, it was maintained that the growth could be kept firm, the top of the tree compact, and the branches well set with buds, all conditions favorable to hardiness. Practically all of the growers report that late fall growths are susceptible to winter injury of both wood and bud.

DO COVER CROPS PROTECT TREES FROM COLD?

There were no conflicting opinions on this point. Growers who had planted cover crops, and nearly all had, were agreed as to the value of this method of protecting trees from winter freez-

ing. Many individual cases were cited of orchards having cover crops surviving this cold winter or that when nearby orchards without the covering crop holding a muffler of leaves and snow were killed. The peach growers in the two regions consider the cover crop the most effective treatment of their orchards to avoid winter-killing, holding that they protect the roots from cold, cause the trees to ripen their wood quickly and thoroughly, and assist in regulating the supply of moisture.

ARE SEEDLING TREES HARDIER THAN BUDDED VARIETIES?

Seedling peach trees are popularly supposed to be hardier than budded varieties. Most of the correspondents in this investigation state that such is the case but none give reasons for the supposed greater hardiness of the seedlings. The statements made are in no way convincing and the greater hardiness of the seedlings can be proved only by carefully conducted experiments. Two hypotheses should be tested in determining whether there is a difference in hardiness between budded and seedling trees: 1. Budding may decrease hardiness. 2. Seeds for the stocks of the budded trees come from the South and these may produce more tender trees than would northern-grown seeds from which seedlings come.

IS THERE ANY DIFFERENCE IN HARDINESS BETWEEN LOW-HEADED AND HIGH-HEADED TREES?

All growers in both States prefer low-headed trees, claiming that both trunks and branches are more often injured in high-headed trees. Buds, however, often survive on the higher branches and not on the lower ones. The reasons vouchsafed for the difference are: the effects of winds in drying out the wood of high-headed trees; low-headed trees are usually most vigorous; and lastly, better protection to the trunk from the sun and hence from sunscald, one of the effects of freezing and thawing. Attention is called by several growers to the fact that buds on high-headed trees usually suffer less from spring frosts.

ARE WINDBREAKS A PROTECTION TO TREES OR TO BUDS?

There was much difference of opinion. From the experiences given it seems that the value of a windbreak depends

largely upon the topography of the land. A windbreak so situated as to form still air can only be detrimental so far as cold is concerned. So planted as to deflect or cause air currents they become of value in keeping off frosts. More often than not, however, it was claimed, they seriously check atmospheric drainage and the damage by frost is increased. Another disadvantage is, should the windbreak be to the north, the buds on the trees thus sheltered are forced and are therefore more liable to injury by late frosts. The testimony was for most part unfavorable to windbreaks.

WHAT DEGREE OF COLD WILL KILL PEACH TREES?

There was a most surprising uniformity in the answers to this question. Nearly all of the correspondents set 20° below zero as the temperature that will kill the peach tree under normal conditions, though some had known them to withstand temperatures of from 20 to 30°, depending upon the condition in which the trees went into winter. The following are the conditions unfavorable to withstanding cold and about in order of the frequency in which they are mentioned: lack of maturity of wood; lack of protection of roots by snow or cover crops; poor soil drainage; overbearing in the preceding crop; lack of vitality from ravages of insects or fungi; and the susceptibility of the variety to cold.

WHAT DEGREE OF COLD WILL KILL PEACH BUDS?

From the answers to this question we are forced to conclude that much more depends upon the condition of the buds than on the temperature, assuming of course a temperature below zero and not greater than 25°, which seems to be the limit that peach buds can stand even under most favorable conditions. The chief factors influencing tenderness of buds are: maturity of buds; variety; and the time at which the buds of a variety finish their resting period and become ready to grow. Some of the factors influencing temperature are: lay of the land; proximity to water; stresses of changeable weather; altitude; latitude; and currents of air.

ARE TREES FROM NORTHERN NURSERIES HARDIER THAN THOSE FROM SOUTHERN ONES?

Many opinions were expressed, but few men had grown trees from different latitudes under such conditions as to answer the question fairly. The answers were in no way decisive and the question is still an open one to be settled only by direct experimentation with trees of the same varieties from North and South grown under identical conditions.

II.

The following variations in the species favorable to hardiness to cold were noted:

DOES THE CHARACTER OF INDIVIDUAL TREES HAVE ANYTHING TO DO WITH HARDINESS?

Answers to this question were very indefinite and often conflicting. It was held by some, and with a fair show of experience to confirm the contention, that trees naturally high-headed with few branches, long, spindling trunks, branches and twigs, have soft wood and are therefore more susceptible to freezing. On the other hand, that individuals having naturally short bodies, a goodly number of branches starting low, with short-jointed wood, bright and clear when cut, and thickly set with buds, were the least easily injured by cold. One tree of a variety may be supposed to be slightly more hardy to cold than another through inherent variation, but whether such hardiness can be detected through the character of the growth would have to be determined by carefully conducted experiments and can hardly be proved by such observations as my correspondents are able to make.

ARE THE SMALL-GROWING VARIETIES WITH COMPACT HEADS HARDIER THAN THE FREE-GROWING SORTS WITH LARGE HEADS?

Practically all growers say that the compact growing sorts are the hardiest. As would be expected the small-headed varieties are those with the least succulent wood. The following varieties are named as being the most compact growers and hence hardier than the average: Hill's Chili, Crosby, Gold Drop, Barnard, Kalamazoo, Triumph, Wager and Fitz Gerald.

IS THE WOOD OF SOME VARIETIES MORE SUCCULENT THAN THAT OF OTHERS MAKING SUCH SORTS SUSCEPTIBLE TO COLD?

Every experienced orchardist or nurseryman knows that there is a great variation in the texture of peach wood. Some varieties have a much more succulent growth than others grown under the same conditions. Succulency of growth is in some cases a well-marked varietal character and one that can be avoided in selecting sorts to plant where hardiness is a requisite. Summarizing the answers from New York and Michigan, the following are the sorts most often named as having the softest and sappiest wood growth: Early Crawford and Late Crawford are named by practically all correspondents as being most succulent in growth, following which, named in order of degree of succulency come: Chair's Choice, St. John, Niagara and Surprise.

ARE YOUNG OR OLD TREES HARDIEST?

Beyond all question young trees suffer most in severe winter freezes. Practically all of my correspondents in both New York and Michigan agree to this, and as a proof many of the Michigan growers give their experience in the several severe freezes that have occurred in that State during the past few years, in which young trees universally suffered most. It is probable that young trees are injured most because they make a much greater and much ranker growth than the older ones and hence more sap remains in them during the winter. The formation of buds in the older trees is helpful, too, in maturing the wood. There are, however, many exceptions to the statement that young trees are less hardy to cold than old ones. Old trees can be forced to produce large quantities of new wood susceptible to winter-killing, while on the other hand the superabundant growth of young trees can be kept down by orchard treatment. It is fair to assume, too, that old trees possessing very low vitality are less hardy than vigorous young trees. Thus it was often noted that old trees which had suffered from the ravages of borers, or fungus parasites, as curl-leaf or shot-hole fungus, were easily killed by cold.

While young trees are more susceptible to freezing than old

ones, yet they are much more likely to recover, if recovery is possible, and their return to the normal condition is more rapid. This is probably true because of the greater vigor of the younger plants and because of the possibility of an entirely new covering of bark for small trees often impossible with larger ones.

NAME THE FIVE VARIETIES OF PEACHES MOST HARDY IN WOOD.

There was, as would be expected, great difference of opinion as to the sorts most hardy. In New York the following five sorts, in order named, were considered most hardy: Crosby, Hill's Chili, Stevens' Rareripe, Gold Drop and Elberta. In Michigan practically every grower considered Hill's Chili most hardy in wood, followed closely by Crosby, then Gold Drop, Kalamazoo and Barnard. It was interesting to note that Elberta, Smock and Salway, considered fairly hardy in New York, are somewhat tender in Michigan. The three upon which growers agree in both States as being hardest are, Hill's Chili, Crosby and Gold Drop. Wager, Jaques Rareripe, Carman, Belle of Georgia, Hale's Early, Champion, and Greenboro, none of them in the lists of five hardiest, are hardier than the average.

NAME THE FIVE VARIETIES MOST TENDER IN WOOD.

Here, too, opinion differed, but not so much as in naming the lists of hardy sorts. In New York the list runs: Early Crawford, Late Crawford, Chair's Choice, St. John, Niagara. In Michigan the first four are as in New York, Early and Late Crawford, Chair's Choice and St. John, followed by Smock, which, strange to say, is considered a fairly hardy sort in New York. Michigan growers consider Salway tender in wood, while in New York there was an even division as to whether it was hardy or tender. Elberta came within a vote of tying Smock for the list of tender varieties in Michigan.

NAME FIVE VARIETIES OF PEACHES MOST HARDY IN BUD.

The New York growers named more than a score of varieties as being hardy in bud and were agreed only upon two sorts as being preeminently hardy, namely: Crosby and Hill's Chili, with Triumph, Gold Drop, Steven's Rareripe and Kalamazoo having an equal number of votes for hardiness. The Michigan

growers gave their opinion most decidedly for the five following sorts, scarcely any others being named: Hill's Chili, Gold Drop, Crosby, Kalamazoo and Barnard, with a few scattering votes for Triumph, Early Rivers, Wager and Salway.

NAME THE FIVE VARIETIES OF PEACHES MOST TENDER IN BUD.

Growers in the two regions agree as to the sorts most tender in bud. Not only are the same varieties given but in exactly the same order, namely: Early Crawford, Late Crawford, Chair's Choice, Reeve's Favorite and Elberta. Among other sorts named as being tender in bud in one or the other or both States are Old Mixon, St. John, Smock, Niagara, Surprise, Globe and Mountain Rose.

In summarizing the results of the investigation it appears that the peach is certainly influenced as to hardiness by the cultural treatment given. The presumption is, upon philosophical grounds, if we accept neo-Lamarckism,—and most horticulturists do,—that the external influences of orchard management have a permanent effect upon hardiness of the peach and that the horticulturist is thus slowly but surely acclimatizing this species to greater degrees of cold than it could once stand. It appears, too, that there are favorable variations in the peach as to hardiness of wood and of bud, from which the horticulturist can select and breed varieties capable of withstanding the vicissitudes of climates which in its wild state this plant could not have borne. We have, in cultural treatment and selection, means at the command of the horticulturist to acclimatize plants, and I have tried to set forth in their relative importance the chief factors as these means are now being used in the acclimatization of the peach.

The substance of the following paper was given by George V. Nash, during the course of an inspection, by members of the Conference, of the collections of the New York Botanical Garden, at the conclusion of the Conference:

Observations on Hardiness of Plants Cultivated at the New York Botanical Garden.

By George V. Nash,
New York Botanical Garden.

During a number of years past there have been grown in the collections of the New York Botanical Garden a large number of species and varieties of shrubs and conifers, and it is observations made upon these that it is desired to place on record here. The unusually severe winter of 1903-1904 will long be remembered by plant lovers in this region, and it has been thought best to disregard in great measure the effects produced by that extraordinary test, considering the various species from the standpoint of their adaptability to ordinary conditions. A detailed account of the effects of this winter upon the shrubs at the Garden was given in the *Journal* of the New York Botanical Garden for July, 1904.

The collection of shrubs which forms the basis of most of the conclusions offered below is located on a flat plain to the northeast of the Museum Building. Here have been brought together over fourteen hundred plants, representing about six hundred species and varieties. The soil is rather light and is underlaid with gravel, so that drainage conditions are excellent. On opposite sides of this tract are depressions of considerable extent, markedly lower than the surface of the plain, thus insuring air-drainage, so that masses of cold air do not collect here. The region is, however, subject to the sweep of the cold winter winds, excepting in the vicinity of a boundary border and bridge-approach on the northwesterly side. It is necessary to describe these conditions that the remarks offered below may be available for the use of others.

It will not be possible to refer to all the plants brought together in this collection. For a detailed account of the behavior of a large number of these during the winter of 1903-1904 the reader is referred to the *Journal* above referred to.

As this collection is for study purposes, it is arranged in botanical families, following the sequence of Engler and Prantl. For this reason, the shrubs will be considered in family groups.

The genus Berberis, the barberries, furnishes a number of species which are perfectly hardy. Among these are B. vulgaris and its purple-leaved form; B. amurensis, from Manchuria and north China; B. aristata, from the Himalayan region; B. buxifolia and its variety nana, from the southern Andes; B. Neuberti, of hybrid origin; and the ever and deservedly popular B. Thunbergii, perhaps the best barberry ever introduced into cultivation. The beautiful little barberry, B. concinna, from the Himalayan region, kills back somewhat at the tips. In a more sheltered situation it would almost surely prove hardy. It is a dainty little species and colors beautifully in the fall.

In the hydrangea family there is Hydrangea quercifolia, from Georgia and Florida, which kills back partly at times. Duetzia crenata and its derivatives are unstable, sometimes killing back to the ground, while D. gracilis and its derivatives are much more hardy. All of the genus Philadelphus, including all of the commoner forms offered in the trade, have proved perfectly hardy.

In the gooseberry family nearly everything is satisfactory. Ribes sanguineum, from the west coast, however, is apt to succumb to exceptional cold, and always is a little unstable. One member of this family of comparatively recent introduction is Ribes curvatum, from the southern Alleghenies. It has proved entirely hardy during our coldest winters. It is most desirable from a decorative point of view. Its branches are long, slender and spreading, giving the plant a very graceful appearance, much resembling in habit Stephanandra flexuosa. During the early summer it is covered with a multitude of dainty white flowers.

In the witch-hazel family Corylopsis spicata, from Japan, and Fothergilla Carolina and F. major, both from our Southern

States, endure our climate well. Hamamelis Virginica, being a native, is of course hardy, but the Japanese representative, H. arborea, is not fitted to our conditions. In the rose family most of the spiræas are satisfactory, a notable exception being Spiræa canescens, from the Himalayan region, which kills back badly, a regrettable fact, for its graceful habit would make it a desirable ornamental shrub.

In the apple family, Pomaceæ, there are many desirable things. Nearly all the thorn apples, excepting those from the extreme south, are available out of doors. In the genus Cotoneaster, however, there is a wide difference in the hardiness of various species. Of those from the Himalayan region there have been grown here: C. Nummularia, C. bacillaris, C. microphylla, C. buxifolia, C. rotundifolia, and C. thymifolia. The first two mentioned have proved hardy, while the remainder are very unstable, even when protected by straw. Perhaps the explanation of this is in the fact that the two first are deciduous, while the others are evergreen, thus presenting a much greater transpiring surface which must act to their undoing in the changeable climate of our winters.

Coming from Schipka Pass, high up in the Balkan Mountains, Prunus Laurocerasus Schipkænsis is tolerable of this climate, but I fear the alternations of freezing and thawing would be its undoing in exposed situations. Of the three species of Cercis grown, the only one which is not satisfactory is the European species, C. Siliquastrum, the other two, C. chinensis and C. canadensis being entirely hardy.

There are so many delightful things in the Papilionaceæ, or pea family, that it is regrettable so few of them are satisfactory in our climate. Caragana Chamlagu, from northern China, and C. arborescens, from Siberia and Manchuria, are desirable sorts. Colutea arborescens, from southern Europe, is also hardy. The European Cytisus capitatus can also be relied upon. Lespedeza bicolor, from Japan, although a showy and desirable shrub, is not quite satisfactory, often killing back badly. While the furze, Ulex europæus, and the broom, Cytisus scoparius, are not at all desirable from the standpoint of hardiness, both killing back badly, even in mild winters.

Of course the Rutaceæ, the orange family, present few species which are hardy in our climate. Notable examples of hardy forms are Ptelea trifoliata and Xanthoxylum americanum. The trifoliate orange, Citrus trifoliata, is just on the borderland, and in this vicinity needs a sheltering hedge for pretection to make it at all permanent.

The boxes, Buxus, do not stand well in exposed places. While some of the forms of the common box, Buxus sempervirens, are better than others, they all do much better when in a protected situation.

To the Anacardiaceæ belong the sumacs, the genus Rhus. Many of these are perfectly satisfactory, including, of course, our native species, R. hirta, R. glabra, and R. copallina. The Chinese R. Osbeckii is especially desirable for foliage effects, owing to its entire hardiness.

The holly family, Ilicaceæ, has some species which are hardy. The Japanese Ilex crenata has proved a most desirable plant with us, even in exposed situations, its dark rich green leaves remaining all winter. Ilex opaca, the American holly, does better when protected, not taking kindly to a wind-swept area. The English holly is, of course, out of the question here.

The Celastraceæ present a varied lot as to hardiness. Euonymus alatus, from China and Japan, is very desirable, both from its beauty and from its ability to stand successfully the rigors of our climate. E. europæus and the American E. atropurpureus are both available. E. japonicus, from southern Japan, as might be expected, is not hardy in exposed situations, requiring considerable protection, while E. radicans, from the middle and northern portions of the same country, can be relied upon.

Stuartia pentagyna, from the southern mountains, really needs the protection of a hedge to be a success, and S. Pseudocamellia, from Japan, is no more hardy.

The Oleaceæ furnish many things which are hardy, the privets as a rule being among this class. The California privet, Ligustrum ovalifolium, was for years considered hardy, but the winter of 1903-1904 proved its Waterloo, plants during that period being killed entirely to the ground in exposed situations,

while it suffered severely even in more sheltered places. L. Quihoui, a shrub with widely spreading branches and thick green leaves, is hardy under normal conditions.

The species of the genus Buddleia, belonging to the Loganiaceæ, are always more or less uncertain, excepting in protected situations. They come up readily from the roots, however, so if they are occasionally killed back they are worth growing, for some of them are very handsome, notable among these being B. variabilis.

Most of the Verbenaceæ, the verbena family, are quite uncertain as to hardiness, but as many of them readily sprout from the roots when killed back, they are of use horticulturally. Vitex Agnus-castus, from the Mediterranean region, Callicarpa Japonica and C. purpurea, the latter from China, Clerodendron trichotomum and C. serotinum, and Caryopteris Mastacanthus, belong here.

The honeysuckle family, Caprifoliaceæ, is almost synonymous with hardiness, for there are many species in the genera Viburnum, Lonicera, Weigela, Diervilla, Symphoricarpos and Sambucus, which even the severest winters do not harm. Abelia chinensis, another member of this family, is not quite hardy, unless in well-protected situations. It is a beautiful little shrub and is well worth giving a protected place.

The collection of conifers is located on a series of ridges and valleys, those requiring some protection being placed in positions which will afford such conditions. The plants are placed singly, instead of in groups, a condition which perhaps must be borne in mind in considering the following remarks upon their hardiness here.

The genus Picea is located on a slope which faces mainly to the northeast, with no protecting fringe of trees on the exposed side, from which direction come the prevailing cold winds of winter, so that the plants are here subjected to as severe a test from this source as could be imposed in this latitude. The area is underlaid with rock, so that the drainage is excellent, with the exception of that portion at the base of the slope to the northeast, where water is apt to accumulate and stand for some time in winter and after heavy showers in summer. In this lower

portion those plants are located which prefer moisture. In this area have been grown for the past three or four years the following: Picea Ajanensis, P. Engelmannii, P. excelsa and many of its horticultural forms, P. Mariana, P. Maximowiczii, P. Omorika, P. orientalis, P. pungens and its horticultural varieties glauca and pendula, P. polita, P. Sitchensis, and P. Smithiana. From our own country come P. Engelmannii, P. Mariana, P. Sitchensis, and P. pungens. Of these the black spruce, P. Mariana, is the only eastern representative which has been grown in the pinetum. It is not entirely at home, although removed but a few miles from a region where it is wild, the unstable temperature of winter here, with the alternate thawing and freezing, apparently not suiting it. The other two referred to are from the west, and are more satisfactory. This is especially true of P. pungens and its varieties. This tree is found at elevations from 6,500 to 10,000 feet in the Rockies in Colorado, eastern Utah, and as far north as Wyoming. It is one of the most desirable American conifers for this latitude, making a fine appearance at all times, not browning in the least during the winters, and in the early summer the glaucous foliage of the young shoots, which is much intensified in the variety glauca, gives a beautiful grey-blue tinge to the whole tree. Picea Engelmannii, reaching its perfection much further north, where conditions are quite different from those prevailing here, is hardy, but does not present that vigorous appearance presented by P. pungens. It grows at an altitude of about 5,000 feet in its northern limit, Alberta and British Columbia, to about 11,500 feet in its southern limit, northern New Mexico and Arizona. In the region where P. pungens is at home, therefore, it grows at an altitude of 1,500 to 2,000 feet higher than that species. This could easily account for the difference in adaptability to this climate. Picea Sitchensis of the northwest coast of North America attains its best development near the sea. That it is not a success in this latitude is not a cause for wonder, the drier conditions here proving a severe check to it. Picea excelsa, widely distributed in Europe, excepting the extreme southern portions, does very well. This has been so long in cultivation that little need be said about it. In Norway, in latitude 63°,

it grows at an elevation of 2,500 to 2,900 feet, while in the Tyrolese and Swiss Alps it reaches an altitude of 6,500 feet. Some of the dwarf forms of this are not as hardy as the type.

Picea orientalis, from the Caucasian region, while hardy, has proved a slow grower. I use the word hardy somewhat advisedly, as of the eight or nine plants set out in 1903, all but three or four died during the succeeding winter. All the plants which survived were derived from one source, perhaps originally from seed from trees growing in a climate more nearly approaching that here. Another species of Europe, with an extremely limited range, is Picea Omorika, confined to the mountains of southwest Servia and the spurs leading therefrom. It grows at an elevation of from 2,000 to 4,000 feet. It is odd in having its nearest botanical relatives in P. Sitchensis, of northwestern North America, and P. Ajanensis, of Japan. Five plants of it have been in the pinetum since the spring of 1903. They have proved perfectly hardy in an exposed situation, having passed through two unusually severe winters during that period. They are trim in habit, a clean green, and keep their branches right down to the ground.

From Japan come two of the species in cultivation. These are P. Ajanensis and P. polita, the former also extending to the mainland in the Amur region. Picea Ajanensis is found in Japan mainly on the island of Yezo, and on the island of Saghalin and the Kurile Islands to the northward, extending southward to about 35° in the island of Hondo. It is said to be particularly at home in the cold swampy plains of the western side of the island of Yezo, and this perhaps accounts for its lack of interest in our climate. With us it has been a slow grower, its location here perhaps being too dry. Picea Maximowiczii, which is said by some to be a form of P. obovata, has proved hardy. Its origin is somewhat obscure, but it is said to have come from Japan. Its nearest relative, however, is apparently P. obovata, a Siberian species. P. polita, now exceedingly rare in a wild state in Japan, but extensively cultivated there, has proved adapted to this climate. Its range, as indicated by the few remaining trees in a wild state, appears to have been in the mountains from the southern part of Japan to as far north as

about 38°. Picea Smithiana, from the temperate Himalayas, has not succeeded well with us up to the present. It grows at an elevation of 6,000 to 11,000 feet, chiefly on the western and northern slopes. It is a curious fact that it frequently occurs with Cedrus Deodara and Pinus excelsa, the first of which has proved a very doubtful proposition in this region, while the latter is hardy.

In the nurseries of the New York Botanical Garden, which are located on ground sloping to the southeast, other species have been grown. But even in this sheltered position Picea Breweriana is a failure. Plants which came into the collections in 1901 have remained almost at a standstill, and are but little larger than when they first arrived six years ago. P. brevifolia, P. Canadensis and P. obovata have proved satisfactory in this nursery. They were moved into the pinetum the past spring, and it will require at least one winter to indicate their fitness for this region.

The genus Abies has essentially the same conditions to meet in the pinetum as has Picea. There have been growing there for the past few years the following species: Abies balsamea, A. Cephalonica, A. Cilicica, A. concolor, A. firma, A. Fraseri, A. homolepis, A. lasiocarpa, A. nobilis, A. Nordmanniana, A. Numidica, A. Picea, A. Sibirica, and A. Veitchii. Abies Veitchii, from an elevation of 7,000 to 8,000 feet, and also known from the Manchurian mainland, and Abies homolepis, from central Japan, at an elevation of 4,000 to 5,000 feet, appear perfectly adapted to this climate. Even the past winter, when some conifers, which had hitherto been looked upon as suitable, turned badly, these two kept green. Abies firma, from further south in Japan, does not show that vigor here that those mentioned above have done.

Abies Sibirica, extending its range as far north as 66°, has a most extensive range. It is found from the northeast part of Russia and eastward through the entire length of Siberia to Kamschatka and the Amur region. This is perhaps a more extended range than is enjoyed by any other species of this genus, but the whole area of its range is known as one of extreme and continued cold during the winter, with sudden transitions from

winter to summer and vice versa. Coming from this region of continuous cold, it is perhaps the alternate thawing and freezing which is destructive to this species. It certainly cannot be the cold here, for in its native home it is subject to far more severe conditions of this kind. It has been tried several times in the pinetum, with poor results.

Abies Nordmanniana has proved quite satisfactory. It is a native of the central portions of Transcaucasia, where it forms large forests in the valleys at elevations ranging from 3,500 to 6,000 feet. It extends as far west as Trebizond, southeast of the Black Sea, in practically the same latitude as is New York City. In passing I would remark that Picea orientalis, which is said to be frequently associated with Abies Nordmanniana in Transcaucasia, is quite variable as to hardiness. Plants derived from some sources have proved perfectly hardy, while others will not stand our winters. These plants were secured from nurseries, and unfortunately it is impossible to obtain data as to where the seeds from which they were raised were originally secured.

Of the firs from western North America three have been under cultivation in the pinetum for several years. These are Abies concolor, A. nobilis, and A. lasiocarpa. The latter is an alpine plant, and like all such plants does not find a congenial home here in New York, the long periods of drought during the summer and the alternate freezing and thawing during the winter militating against them. It is not satisfactory, although able to live through the winters. Abies concolor, much resembling it in color, is much more satisfactory and is one of the best of American conifers for this vicinity. Its ability to withstand heat and dryness, makes it especially valuable. Previous to the past spring, I do not recall its having shown any signs of browning, but some of the specimens did brown rather badly the past winter, while others kept their beautiful gray-green without a blemish.

Abies nobilis, which attains a height of 150 to 250 feet in its native home, the Coast and Cascade ranges of Washington and Oregon, is a very slow grower here, perhaps being drawfed by the drier climate of this region. It is perfectly hardy, how-

ever, even in exposed situations, not browning nor killing back in any way during the most trying winters.

The two firs of the eastern parts of North America have both been under cultivation at the Garden for several years. Both of these, Abies balsamea and A. Fraseri, the latter restricted to the southern mountains, are not desirable as ornamental trees. With us they are slow growers and of doubtful stability, the alternate freezing and thawing, perhaps, being the cause of this.

Abies Cephalonica, from elevations of 2,500 to 5,000 feet in Greece, stands the winters well. A. Cilicica, from altitudes of 4,000 to 6,500 feet in Asia Minor, is also hardy. This is another example of the strange difference in hardiness in this latitude of two plants which are often associated together in a wild state. Abies Cilicica and Cedrus Libani are said to be constantly found growing together, and yet the latter has proved entirely unfit to stand our winters, while the Abies has been grown in an exposed situation with success for the past four years. Abies Numidica, an associate of Cedrus Atlantica in the Atlas Mountains, has been represented by a single specimen in the pinetum for the past four years. It is still in good condition, but it does not grow very fast. Abies Picea, or A. pectinata, as it is more frequently called, is the common silver fir of central and southern Europe. It is said to attain its greatest development in the humid mountain tracts of central Europe, a reason, perhaps, for its unsatisfactory behavior here. It comes through the winters alive, but it is apt to brown badly, and does not grow rapidly.

Pseudotsuga mucronata, which has, perhaps, as wide and extensive a range as any American conifer, is one of the best for this latitude. Its wide range indicates its ability to adapt itself to a variety of conditions, and it makes one of the best and handsomest trees in this neighborhood, not even the most severe winters harming it in the least. Its dark green foliage is a delight in the winter time, and in the spring the fresh green of the young shoots in contrast with the dark green of the older branches, makes it most attractive.

The pines, often residents of dry or cold regions, lend themselves more readily to cultivation in this region than do

most other conifers. Among the white pines, Pinus excelsa, of the Himalayan region, growing at an elevation of between 6,000 to 12,500 feet, is perhaps the best. Its leaves are longer and more graceful than are those of our own white pine, Pinus Strobus, and it is less subject to disease. Pinus Koraiensis, the Corean white pine, and said also to grow in Japan and China, is a rather slow grower, but perfectly hardy. Pinus parviflora, ascending the mountains to about 5,000 feet in central and southern Japan, thrives well here, and is desirable where a slow-growing tree is wanted. Pinus Cembra, of central and northern Europe, and Pinus Peuce, of Macedonia and Roumelia, are both hardy but slow growers.

Among the red pines, Pinus Thunbergii, of Japan, and also said to occur in north China and Corea, rivals Pinus Austriaca in hardiness. It much resembles it in general shape. Another desirable Japanese pine is Pinus densiflora, of central Hondo, where it grows among deciduous trees at an elevation of 3,000 to 4,000 feet. Pinus Laricio, P. Austriaca, and P. Pallasiana, of the mountains of southern Europe, the latter two often considered but as varieties of the first, stand well here. Pinus Banksiana, P. montana Mughus, P. pungens, P. resinosa, P. rigida, and P. sylvestris all thrive. Pinus ponderosa is not really at home, at least the plants we have, some six or seven, are not vigorous. Pinus Taeda is barely hardy in sheltered places, the young growths often killing back.

Cedrus Deodara, growing in the Himalayan region at elevations of 3,500 to 12,000 feet, barely survives, although Pinus excelsa, with which it is said to be associated in Cashmere, is perfectly hardy. We have plants in the pinetum which came to us in 1900 and which are still small, showing a tendency to kill to the snow line in severe winters. Cedrus Atlantica, from the Atlas Mountains, where it ranges between elevations of 4,000 to 6,000 feet, is much better adapted to this climate. Here we have an interesting case of two species of the same genus, in practically the same latitude, about 32° north, one in the northwestern part of Africa, the other in the northeastern part of India, the African species being much more hardy in this cli-

mate than the Indian species, although it grows at a lower general altitude.

In the genus Larix, as might be expected, Larix leptolepis, from the mountains of northern Japan, is most satisfactory. The severest winters we have had have not touched it. The common European larch, Larix decidua, and the American one, Larix laricina, are, of course, hardy. Pseudolarix Kæmpferi, of China, has proved suited to the climate in this neighborhood. As both Larix and Pseudolarix are deciduous, they are better adapted to stand the alternate thawing and freezing they encounter here, for the transpiration surface is reduced to a minimum.

It is unfortunate that Cryptomeria Japonica is not hardy here. We have tried it several times, and only once have we found a single individual that would live at all, the others being killed every winter. This plant has now been in the pinetum for five or six years, is in a very sheltered place, and has managed to survive; it does not present the neat symmetrical appearance it does in the conservatories. It has borne cones and even the hard winter did not harm it more than usual.

Sciadopitys verticillata, from the mountains of Japan, withstands the winters well, rarely if ever browning in any way. Thujopsis dolabrata, as is the case with nearly all plants from southern Japan, is not hardy here. Chamæcyparis pisifera, and its numerous varieties, also from southern Japan, is a little tender sometimes. Even here there is a great difference in individuals, some withstanding the winters better than others, perhaps being derived originally from seed from more northern localities. Chamæcyparis obtusa, and most of its varieties, are about as hardy.

None of the species of Cupressus are hardy. The members of the genus Chamæecyparis from our northwestern country, C. Nootkatensis and C. Lawsoniana, can only be grown in sheltered situations. Coming from a region where the annual precipitation is greater than it is here, our long dry spells seem to militate against them.

Among the cedars, Juniperus chinensis, J. nana, J. Sabina, J. prostrata, and of course the native J. Virginiana, are per-

fectly hardy. While J. rigida, one of the most graceful of all, at least in a young state, from southern Japan, is only capable of a struggling existence, and winter-kills badly.

Thuya occidentalis is hardy, excepting in wind-swept situations, where it often kills badly and browns. Thuya gigantea, from the northwestern coast of America, where humid conditions prevail, will not stand here at all, and Thuya orientalis, of China, is precarious excepting in sheltered localities.

Taxodium distichum and T. imbricarium both thrive, whether grown in wet or dry soil.

Tsuga canadensis, of which a fine grove is to be found along the banks of the Bronx River in the New York Botanical Garden, is of course hardy. T. carolina, of the southern Alleghenies, is equally hardy. T. Mertensiana has been tried twice and both times it failed, the plants when set out being in an apparently healthy condition, the first winter being fateful to them.

Ginkgo biloba thrives vigorously, the coldest winters not killing even the smallest branches. This cannot be said of the genus Taxus, however. The common European yew, Taxus baccata, needs a protected situation to enable it to pull through a severe winter. The Japanese yew, Taxus cuspidata, however, is perfectly hardy, and during the severe winter of 1903-1904, when the European yew was killed, in many cases to the snow line, T. cuspidata, growing immediately alongside of it, was not hurt in the least, but kept green and intact the whole winter.

Cephalotaxus is represented in the collections by two species, C. drupacea and C. Fortunei, the former from Japan, the latter from China. C. drupacea grows in the mountains, at elevations of 1,000 to 3,000 feet, from southern Hondo to central Yezo, often forming a part of the undergrowth in woods. C. Fortunei is from the northern part of China. Both these species can only be grown in this neighborhood in protected situations, and even then present rather a scraggly appearance.

In this connection, as associating the data with horticultural matters, I desire to place on record the following, compiled from the records kept at the New York Botanical Garden for a number of years. The precipitation is given for what may be called the eight growing months of the year, from March to October,

inclusive. This may throw some light upon the problems of hardiness of certain species, when more is known as to their individual environment.

	1901	1902	1903	1904	1905	1906	1907
March	6.89	5.63	5.96	3.82	4.47	4.15	2.31
April	8.96	3.73	3.49	5.00	2.88	6.50	4.93
May	8.08	1.85	.34	4.11	1.05	4.61	4.05
June	1.04	5.65	8.28	2.6	4.01	1.71	3.85
July	11.76	4.12	5.34	3.59	4.13	4.12	1.66
August	8.56	5.75	5.94	6.52	6.04	3.78	2.59
September	2.23	5.83	3.6	4.06	6.09	2.53	7.93
October	3.21	7.31	8.98	2.77	2.87	5.81	
	50.73	39.87	41.93	32.47	31.54	33.21	

It will be noted from the above that from 1901-1906 a period of unusual dryness has visited this region either in the latter part of May or during June. In 1907, however, this drought appeared later, coming in July. It would seem that it is this dry period which militates so strongly against growing plants here from a humid region, such as many parts of the Pacific coast, and if we can but tide our plants over this period each year by carefully watering them, we may eventually succeed in establishing many a plant which otherwise would succumb to the dry conditions of its first year of residence, and eventually perhaps establish a vigor which may perpetuate it. This is particularly true of conifers, for it is just about this time that such plants are transplanted in the vicinity of New York. One can readily imagine the effect which would be produced upon a conifer which was transferred from a nursery to a new situation just previous to the commencement of this drought. Perhaps many of our failures with conifers is due to this cause. A conifer handicapped with such conditions for several weeks after transplanting has but a poor chance to recuperate and lay up reserve force to carry it through a severe winter. I can recall one excessive visitation of this drought in 1903, when the precipitation for fifty-two days, from April 16th to June 6th, was but 0.37 of an inch. This was followed by the extraordinarily severe winter of 1903-1904, and the resulting havoc in shrubs and conifers will be long remembered by

many a lover of hard-wooded plants. The results upon shrubs at the Garden have been already referred to.

The alternate thawing and freezing of this vicinity must also play an important part in the hardiness of plants here. Alpine plants, or those from regions of perpetual cold during the winter, do not therefore adapt themselves readily, nor do they take kindly to the succeeding fierceness of our summer sun, after a winter of subjection to thawing and freezing.

There is hardly sufficient data as yet on which to base a statement as to what plants are hardy in the vicinity of New York. Not until many investigators have recorded the results of numerous experiments in various localities, and not until we know more about the individual environment of a given species, can we with any certainty explain the apparent contradictions which seem to exist in the matter of plant hardiness. In so far as the shrubs and conifers are concerned, it may be said that, as a general rule, species from the Alleghenies and from the regions somewhat to the north of New York are hardy here, the belt of hardiness extending across the northern border of the United States to the Rockies, and extending southward down them at elevations of medium height; while plants from the Pacific coast, even as far north as Washington and Oregon, lead a very precarious existence in this latitude. In Asia, hardy species come from northern and middle Japan, northern China, Manchuria and Siberia, with a few from the Himalayan region. In Europe, of course many of the plants from the northern and middle portions adapt themselves to conditions here, and from the high mountainous regions of the southern portions come some of our best conifers, while but few plants from the region of the Mediterranean survive long.

Observations in the Region at the Head of Lake Michigan.

By Jens Jensen, *Chicago, Illinois.*

Generally speaking the topography of the district under discussion in the following notes is level. Geologically the region at the head of Lake Michigan consists of the following formations:

First.—Alluvial Deposit.

Second.—Glacial Drift.

Third.—Morain.

The *Alluvial Formation* consists of a series of sand ridges that form sand dunes toward the northern part with intervening spaces, still inundated or here and there raised up above the Lake level by decaying vegetation. All of the dry lands are covered with forest growth.

The *Glacial Drift,* known to geologists as Lake Chicago, but commonly termed "Prairie," consists of a heavy blue clay, and was entirely treeless before the arrival of the white man in this section. Judging by its name it is almost level, and has a poor natural drainage.

The *Morain* borders on Lake Michigan north of Chicago, here known as "Lake Border Morain," and also borders the "Prairie," or "Glacial Drift," described before. This formation consists of a yellow, pebbly clay, and was originally covered with forest growth, part of which still exists where the axe has left it alone.

The elevation over Lake Michigan of these three different formations varies from a few feet to 180 feet, the Morain being the highest, with the exception of a few sand dunes that perhaps reach still a greater height, but which will not be considered here.

From the foregoing description of the formation of the lands at the head of Lake Michigan it at once becomes evident that there exists a difference in the vegetation covering these

areas. Nevertheless, leaving everything but the arborescent flora out of consideration, the difference is very slight, from the standpoint of the great variation in soil, which will be seen later.

For convenience sake the vegetation will be divided into three groups:

First.—Vegetation indigenous to the above described area.

Second.—Vegetation indigenous to the area, but introduced from one formation to another.

Third—Vegetation introduced to the region.

The greater variety is found on the alluvial soil, and some species are found here of more than common interest. Especially is this true of the deciduous group, there being very little difference in the Conifers, all of the latter having been practically exterminated by the soft-coal smoke of the city of Chicago where manufacturing has encroached upon adjacent territory. Originally the same vegetation as now found in the southern border of the city extended along the present water-front of Chicago as far as the northern part of the old city limits.

Of those species indigenous to the Alluvial Formation that will be discussed here are:

Liriodendron Tulipifera.
Nyssa multiflora.
Nyssa sylvatica.
Sassafras officinale.
Fraxinus quadrangulata.

Sassafras is found for about three miles beyond the northern limit of the Alluvial Deposit on the "Lake Border Morain."

Fagus ferruginea is a native of both the eastern and western borders of Lake Michigan, but only two groves are found on the "Lake Border Morain" in the State of Illinois, and also sparsely found in the northern part of the Alluvial Formation on the Michigan side within the limits of the area here discussed. Farther north on both sides of the lake large groves of well-developed trees exist.

Of the Morain vegetation of interest here are:

Acer saccharinum,
Acer rubrum,
Prunus serotina,

all of which are indigenous on the Alluvial Formation, but less vigorous and of smaller growth there.

On the Glacial Drift (Prairie) as mentioned before, the present vegetation has been introduced. Perhaps a lonely cottonwood might have made its home in suitable situations on these plains, as we still find them to-day seeding themselves along ditches and roadways; still it is problematical whether the young sapling would have been able to withstand the annual fires that with great fury passed over these flat lands.

Of course, groves of trees and shrubbery vegetation originally existed along the bluffs of the Chicago River, but these have long ago disappeared with the exception of those along the north fork of the Chicago River, most of which passes through the Morain Formation.

Referring to the second group: those species introduced to the Morain from the Alluvial Deposit will first receive attention. To my knowledge the tulip tree (*Liriodendron Tulipifera*) has been planted as far north as Waukegan, which is about thirty-five miles from Chicago. Sassafras and Nyssa multiflora have also been introduced, but no species of any size worth mentioning exists outside of its natural distribution.

Some healthy species of the tulip tree are found within a short distance of Lake Michigan, and especially noteworthy are the two remaining trees in the cemetery in the city of Waukegan, planted by the late Robert Douglas about forty years ago. The sister specimen of these trees once decorated the beautiful grounds of Mr. Douglas, but was winter-killed in the winter of 1898 and 1899. One large specimen of sassafras, more than twelve inches in diameter, is found directly at the foot of an old lake beach where the Alluvial Formation and the Morain join north of the city of Chicago. As this tree stands in a private garden it is evident that it was planted perhaps between thirty and forty years ago; but native specimens are still found in this district, yet nowhere over ten to fifteen feet high, and this size very sparsely. The tree referred to is protected toward the north and west by the bluff and a group of conifers.

Nyssa multiflora is not found in well-developed specimens outside of the Alluvial Deposit; a few specimens have been re-

ported from the northern section of the Alluvial Deposit in Illinois. Whether those growing in the northern part of the city are native, or have been introduced, I do not know.

Of the vegetation introduced to the Prairie indigenous to the region, I desire to mention:

Fraxinus quadrangulata.
Tulip Tree.
Acer saccharinum.
Acer rubrum.
Prunus serotina.

We will now come to the third group—Introduced Vegetation.

Referring to the Morain,—

Magnolia hypoleuca,
Gleditsia triacanthos,
Ulmus campestris,
Acer platanoides,

have been introduced with more or less success. The magnolia can be found in specimens almost twelve inches in diameter in old gardens as far as Waukegan on the "Lake Border Morain." Gleditsia triacanthos is also found in large specimens, and the Norway Maple grows very rapidly, and attains an enormous size on this formation. Ulmus campestris is dying out after about thirty years of growth, but disseminating itself through seeds before it succumbs to natural conditions deadly to its existence.

On the Plain or Glacial Drift the same varieties have been introduced, and those specimens still remaining show the hard struggle they have to make for existence. Magnolia hypoleuca grows very slow, and as no specimens exist over fifteen years old it is impossible to state at this time how long they will be able to live on this formation. Acer saccharinum succeeds very poorly, and Acer rubrum after a few years of trial succumbs. Acer platanoides (Norway Maple) struggles along and has so far succeeded in holding its own under favorable conditions, but does not attain a height over 25 or 30 feet.

Prunus serotina is not much better off than Acer rubrum. Gleditsia triacanthos grows into large specimens, and seems

to succeed as well on this formation as any other tree if we except the cottonwood, yet in the cold winter of 1898 and 1899 referred to before a number of beautiful specimens of Gleditsia triacanthos were winter-killed on these lowlands. Fraxinus quadrangulata, after a period of thirty-five years, is not more than from nine to ten inches in diameter, and about 18 to 20 feet high, but seemingly healthy, only dwarfed. Nyssa multiflora and Fagus ferruginea die out after a few years of struggle. As most of this formation is within the influences of the smoke of the city of Chicago, no reference will be made to evergreens, as all of them succumb after a longer or shorter period.

In discussing Introduced Vegetation I also desire to call attention to a specimen of yellow wood *(Cladrastis tinctoria)* that stands on the top of the Morain about sixteen miles west of Chicago. This tree is more than twelve inches in diameter, and the only one found of any consequence in the entire region. Introduced to the Glacial Drift it lingers along for a few years and then succumbs. That no reference has been made to Introduced Vegetation on the Alluvial Formation is largely due to the fact that these formations have either been covered with manufacturing, or with the homes of the laboring man where inhabited, and consequently no large gardens are found in this district that naturally would contain a variety of Introduced Vegetation.

Most of the species introduced to the Glacial Deposit are found in the parks of Chicago. I may also here mention the attempt made of introducing Rhododendrons and Azaleas to our parks and gardens, but always with failure, even when the greatest care and study as to soil and natural growth have been considered.

CONCLUSIONS.

Generally speaking, as referred to before, all the species under discussion are found both on sandy and clay soil, consequently the soil conditions need not be considered as to the life of the tree. If we examine the vegetation mentioned on the Alluvial Formation more closely we will find that, following the shores of Lake Michigan from the north on the eastern border toward the head of the lake, the specimens become smaller in

growth, and less vigorous. Almost identically like the red cedar, which in the Middle and Southern States is a large and vigorous tree, gradually becomes smaller and smaller toward the north until it disappears as a low scrubby specimen.

Those trees referred to appear also more sparsely toward the head of the lake. Reference is here made to the Sassafras, the Tulip Tree, Nyssa multiflora and the Beech. Tulip Trees introduced into the more fertile Morain are winter-killed, except when planted under very favorable conditions, and even then are subject to being killed by frost at any time. Sassafras comes under the same class.

That the two trees in the Waukegan Cemetery survived the winter of 1898, and the one on the grounds of Mr. Douglas was killed, was due to the fact that this killing freeze was followed by severe rains. The Douglas tree stood on level ground, and consequently had a wet foot. Those in the Waukegan Cemetery were planted near the gutter of a roadway considerably lower than the point at which the trees stood, and the soil absorbed but little of the rain that fell. It is also evident that where the trees have been planted closer to the shores of Lake Michigan, and thereby subjected to the lake and the moist-bringing lake fogs, they survive better than those farther inland. A study of the natural vegetation within a few hundred feet of the shores of Lake Michigan will tell the story.

The Beech *(Fagus ferruginea)*, found in groves twenty or more miles north of the Wisconsin State line, has been distributed toward the head of Lake Michigan within a short distance of Chicago. The two groups, one west of Waukegan, and one at Highland Park, twenty-four miles north of Chicago, are supposed to be native. The other tree referred to is found in a private garden, and smaller specimens are found in gardens farther toward the head of the lake.

The late Thomas Douglas, who was born in Waukegan when Indians still existed within a short distance of the city, at that time a village, was of the opinion that the Beeches at Highland Park had been brought there by the pigeons which at that time, and still later, infested these forests by the millions. The pigeons were fond of beech-nuts, and it is not at all im-

probable that nuts could have been dropped by the birds, but why do we not find groves of Beeches or scattered specimens farther south? The pigeons were plentiful everywhere at the head of Lake Michigan in those days, and even many years later, but there are no Beeches except what have been planted by man. Another story has it that those trees were planted by the Indians, and I am of the opinion that the Highland Park grove, and those west of Waukegan, were planted by the Indians who frequented these regions very much, and at Highland Park held council. The trees stand close together in a small grove, and cannot be compared with the natural groves north of the State line in height or dimensions. I do not think any of them measure twelve inches in diameter.

The tree referred to farther south within a few miles of Chicago is still smaller and more scrubby in growth, and after forty years has attained a height of less than twenty feet, showing that as we advance toward the head of the lake the Beech becomes smaller and finally disappears.

Referring to the yellow wood mentioned before, its existence is partly due to the fertile and well-drained Morain, and partly to shelter toward the north and west.

Considering the Glacial Drift in the lowlands, we must soon come to the conclusion that this kind of heavy, poorly-drained soil is not adapted for a great variety of trees and shrubs. These lands have not been subject to oxidation for as long a period as the Morains, that have been above water long before the plains arose above the surface of Lake Michigan.

We have seen that the nearer we get to the head of Lake Michigan the less possible it becomes for those specimens described before to live, whether these are planted on the same formation or not. So it is with the Beech and Tulip Tree, native to this region, and many other species introduced; and why is this so? Study for a moment the map of the lands bordering Lake Michigan, and consider that our hot winds come from the southwest across the plains, and our cold winds from the northwest, also across the plains. That these winds crossing great land areas must be dry is evident. The farther we move to the north on either side of Lake Michigan away from

the head of the lake, the more we receive these winds across the lake, and especially is this so on the eastern side of the lake, where both the southwest and the northwest winds must cross the lake. That the lake will have a tempering and a moisture-bringing influence on these winds must be evident.

That the changes brought about are remarkable we all know, and that part of southern Michigan bordering on Lake Michigan would never be the fruit-bearing country it is to-day if it were not for the effects as stated before. The Beech and the west side of the lake is benefited in the same way, and more so where the influences of Lake Superior are perceptible, thereby changing the character of the northwest winds, and this is where we extend into the so-called White-Pine Belt. That the better-drained Morain and the greater fertility of these lands must be considered when compared with the low, poorly-drained plains, and the low, fertile, sandy lands is evident, and the foregoing notes have shown this fact. That the Tulip Tree at the head of Lake Michigan has reached its western limit in this latitude must be conceded, and this is true with many other introduced species. So it is that Rhododendrons and Azaleas that beautify the Eastern parks and gardens are barred from our plains, not on account of colder climate, not on account of soil conditions, but on account of the dry winds in extreme cold and extreme hot weather that sweep across our western plains, and this is the only reason that so many beautiful trees and shrubs are barred from our parks and gardens.

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